PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. III. 1856–57. No. 47.

SEVENTY-FOURTH SESSION.

Monday, 24th November 1856.

VERY REV. PRINCIPAL LEE, V.P., in the Chair.

The following Council were elected:—

President.

SIR T. MAKDOUGALL BRISBANE, BT., G.C.B., G.C.H.

Vice-Presidents.

Sir D. BREWSTER, K.H.
Very Rev. Principal LEE.
Right Rev. Bishop TERROT.
Dr CHRISTISON.
Dr ALISON.
Hon. Lord MURRAY.

General Secretary,—Professor FORBES.

Secretaries to the Ordinary Meetings,—Dr GREGORY, Dr BALFOUR.

Treasurer,—JOHN RUSSELL, Esq.

Curator of Library and Museum,—Dr DOUGLAS MACLAGAN.

Counsellors.

Hon. B. F. PRIMROSE.
JAMES CUNNINGHAM, Esq.
Dr GREVILLE.
A. KEITH JOHNSTON, Esq.
Dr MACLAGAN.
WM. SWAN, Esq.
Dr TRAILL.
Hon. Lord NEAVES.
Dr THOS. ANDERSON, Glasgow.
Rev. Dr. HODSON.
ROBERT CHAMBERS, Esq.
J. T. GIBSON-CRAIG, Esq.

Monday, 1st December 1856.

THE RIGHT REV. BISHOP TERROT, Vice-President, in the Chair.

Opening Address. By Bishop Terrot.

The Council of the Royal Society have, in the course of the last year, taken into serious consideration the state and prospects of the Society, and have deliberated upon many propositions having for their object to render our ordinary meetings more interesting, and generally to increase the efficiency and usefulness of the Society. Among other schemes of this nature, they have adopted one, in conformity with which I have now the honour of addressing you. They have resolved that henceforth the winter session shall commence with an Address from the President, or one of the Vice-Presidents, leaving it, of course, to the judgment of the individual selected to choose such topics as he may think most likely to excite, among the Fellows, a deeper interest in the welfare of the Society, a more earnest determination to render their scientific attainments, whatever they may be, serviceable, not merely to the world at large, and to the advancement of their own scientific reputation, but also to the efficiency and reputation of our common object of interest—the Royal Society of Edinburgh.

Possessing, as we do, a President equally distinguished for his science and for his love of science, for that which he has done himself, and for that which, by his open-handed liberality, he has invited and enabled others to do, it would have been most natural and desirable that he should have been requested to address you on this occasion. You are aware, however, that the state of his health is such as to render it very unadvisable that he should attend our evening meetings, and the Council were consequently under the necessity of laying this duty upon one of the Vice-Presidents. There also, you must be aware, the Council had but a small range of choice. Your senior Vice-President, to whom every one would have listened with interest and attention, and from whom even the most advanced in science might have learned something new, is, I regret to hear, obliged to retire to the Continent on account of the health of a member of his family. Others on the list are prevented by various causes from attending our meetings; and the result has been that the Council felt itself under the necessity of imposing upon me the duty, which I now attempt to perform.

I think you will agree with me, that a Society like ours, instituted for a definite purpose—the advancement of Science and Literature—has, in relation to that purpose, a duty to perform; and that each member, according to the extent of his ability, is bound in duty to contribute to the energy and action whereby alone the Society, as a whole, can satisfactorily perform its obligations. Now, in relation to the purpose of the Society, the clear understanding of which must go far to determine the character of its duties, it cannot, I think, fail to strike you that for many years our attention has been exclusively directed to what is commonly called Science,—to the abstract, the physical, and the experimental sciences; while every thing coming under the designation of Literature is altogether absent from our proceedings. Literature, indeed, in its ordinary acceptation, is unsuited to our purpose. It is not desirable that, like the old French Academy, we should invite poets to recite fragments of some forthcoming epic, or historians to put us in advance of the external world, by communicating the *purpurei panni*, the heroic portraits, or battle-pieces, of a projected narrative. But I cannot see why the word Science should be restricted to the knowledge of material objects; why it should not be extended to all knowledge difficult to acquire, and relating to matters which are interesting to any considerable number of thinking, cultivated minds. It would surely be unjust to refuse the name of science to that philosophy which, in the hands of Smith, Reid, Stewart, and Brown, has done more to raise the character of Scotland as an intellectual nation, than all that she has done, and that is not little, in all the mathematical and physical sciences. And besides this science of mind, there exists a science of the great exponent of mind, of spoken or written language. This, in combination with anatomy, constitutes the science which the British Association have admitted into their cycle under the name of Ethnology. Even those among us who are most absorbed in abstract or physical science may feel some interest in learning to what extent, and how and why it is that a basis of sameness exists between the languages of a long line of nations extending from the Ganges to the Atlantic; how varieties of this one species have branched off in different directions; how in some countries invading hordes have speedily obliterated the language of the conquered, while in others they have abandoned their own, and have adopted that of the conquered majority.

Now, I beg you to understand that I have no wish that either mental philosophy or philology should, in this Society, supersede those abstract and physical sciences with which we are generally occupied. All I would ask for is, that those who are engaged in the former should not be led to consider the Royal Society as an Institution in which they are not wanted, and in whose labours they can take no share. It may be replied, that there exists among us no exclusion of such students, and that our door is as open to them as to the Chemist or the Mathematician. There is no exclusion, but there is an obstruction. Such students, the speculators upon mind and language, when hesitating whether they shall propose themselves as candidates for admission into the Royal Society, will naturally look into some recent volume of our Transactions. There they will find little that can interest them—little that they can understand, or even read. For the notations employed by the analyst and the chemist are to them an unknown tongue; and though they be familiar with common arithmetic, they would not find a volume of the Makerstoun Observations very inviting. In short, the impression would be that our trade was not their trade, or that for their ware there was no demand in our market.

And yet this last conclusion would be a false one. There exists in the Society a very general wish for some infusion of literature into our proceedings, and it exists among those who are themselves most exclusively devoted to scientific pursuits. But it does not lie with the Anatomist, the Botanist, or the Astronomer, to supply the deficiency. All that they can do is to express not merely their willingness, but their wish, that men of letters would come forward and contribute something of a more general interest, and a more graceful character, to the severe simplicity of our usual evening engagements. About this time last year the Council issued a report to the Fellows, in which the subject to which I am now referring was urged very strongly. They then said, "It has long been a matter of regret that literary papers are so seldom offered; insomuch that it is often forgotten that the Royal Society was originally instituted for the interchange of literary as well as of scientific communications; indeed, that the Society long divided itself into two classes, having reference to those subjects. Essays on criticism, philology, and æsthetics, are to be found in the earlier volumes of the Transactions, but for many years such papers have rarely been communicated. The Council believe that such contributions would be very acceptable to the Society, even should the authors not in every case deem their observations sufficiently original and important to demand publication *in extenso* in the Transactions." Such was the urgent call made by the Council upon the literary members of the Society; but as yet the call has been made in vain.

I venture, in the next place, to offer a few words of counsel to those who are so far engaged in science as to be conscious that, by the withdrawal of a little time from their professional engagements, they might contribute either to the interest of our evening meetings, or to the contents of our annual publications. I distinguish between these two purposes, because I consider them widely separate. The ideal of a paper for the Transactions is, that it contains some new important truth; and since, by its publication, it is presented to the whole scientific world, it is clear that the author should have such knowledge not only of his branch of science, but also of its history, as may secure him from wasting his time upon the discovery of that which is already familiar to the masters of the craft. Such, I repeat, is the ideal of a paper for the Transactions. In many cases such a paper can give no gratification to the audience, and, indeed, in the very best cases such papers are not read throughout. A brief abstract of the purpose, method, and conclusion is all that is given; because the author is aware that, to a large portion of his hearers, the details would be uninteresting, because unintelligible; and that even those who are on a level with himself, require time fully and clearly to apprehend the accuracy of his arguments and of his calculations.

But no such requirements and limitations apply to communications made at our ordinary meetings, without any view to their being afterwards published in the Transactions. Those among us who are employed upon sciences of observation, and those who are watching the progress of science both at home or abroad, might add much to the interest of our meetings by communicating information which is not positively, but only relatively new; which might be found elsewhere, but which would probably not be found by many who would gladly receive it, when presented to them by an intelligent informant. Nay, I do not see why the proposing of well-considered questions might not be considered relevant to the purpose of our evening meetings. Those who, from heavy professional occupations, cannot advance beyond the outskirts of any science, would be very troublesome members of society were they perpetually invading the studies of the learned, and applying for the solution of their doubts and difficulties. Yet such sciolists, among whom I must honestly class myself, have, in virtue of their fellowship, some claim upon the assistance of their more learned brethren; and that assistance might be easily afforded, if the proposing of a reasonable question, not pointedly addressed to any individual, were to be considered as an allowable and ordinary proceeding in our evening meetings. These suggestions may appear trifling or impracticable. My purpose will be served if only the attention of the more earnest working members of the Society be turned to the fact that the proceedings at our meetings possess little attraction for a great portion of the Fellows; and if they are led to devise some better plan for popularizing, without degrading, the public business of the Society.

I suppose, that if any of us were asked, What is the purpose of the Royal Society? he would answer generally, the promotion of science. But this formula, the promotion of science, may be taken in various senses. In one sense, and that the highest, a philosopher promotes science when he observes and publishes facts unknown before, or when he reduces known facts under the conditions of a new law. In either case he promotes science by increasing the number of things which may be known by study alone without invention. But the schoolmaster, in another sense, promotes science when he excites to the pursuit of science minds which, without such excitement, would have remained trifling or inert; when he smooths difficulties which would have discouraged, or altogether stopped, the progress of the young student; and in some, though certainly in a much lower degree, when he merely communicates to his pupils his own knowledge of the facts and laws of nature. The philosopher, in the successful exercise of his vocation, makes things knowable; the schoolmaster, in his vocation, makes them to be actually known. So far as I can see, these are the only two methods in which science can be directly promoted; and the question is, in which of these ways is it that our Society ought to labour for the promotion of science. Individual members then, contribute to the advancement of science, when they communicate to the Society either the unrecorded facts which they have observed, or the results of their scientific experiments, or the general laws which they have established by processes of inductive reasoning, or improvements which they have effected in the instruments of observation, or in the calculus by which their reasonings are effected. The Society again, as a body, co-operates to this direct advancement of science, when, after winnowing the important from the unimportant, it gives to the world in its Transactions, such communications as, in its judgment, are fitted either to extend the field or to facilitate the acquirement of useful knowledge. But perhaps the indirect action of this and similar societies is more important than these its formal and visible products; nay, I know not whether the best answer to the question, What is the use of the Royal Society? would not be, that it is useful by bringing together, into familiar intercourse, men of science and men of letters—men of similar and of different views. Solitary study is requisite even for the most moderate attainment of knowledge; but a solitude unbroken by intercourse with other minds, is apt to generate, in scientific men, an overestimate of their own powers and performances, and a doting fondness for notions which are commonly described by the term crotchets. Now every man of vigorous and inquiring intelligence, and so far constitutionally qualified to become a man of science, who, by being brought into competition with his equals, and under the influence of his superiors, is induced to moderate his self estimation, and to abandon his crotchets, is thereby rendered a better, wiser, and more useful man than he was before. I need not again refer to the more obvious use of familiar intercourse among the professors and the lovers of science, of the labour and time that may be saved by the friendly communication of difficulties, or of the overcoming of difficulties, and by everything which tends in science to that generous, unselfish co-operation, which is the source of strength and progress in all artistic, commercial, and social life. Every great subject has some dark side; and, next to the unholy contests of intolerant religionists, I know nothing more melancholy than the disputes of men of science respecting priority of invention and discovery; to see them too evidently acknowledging, that not the discovery of truth, but the credit of having discovered it, is the stimulus and the reward to which they are looking.

The affording facilities for such intellectual intercourse between those who are engaged either upon the same or upon different branches of science, and the promotion of this generous, brotherly co-operation, is, I believe, in the present state of society, the most important purpose, and the most beneficial result of scientific institutions such as that which I have the honour of addressing.

The Council have, in the last year, acted as if they felt the force of some of these considerations. They have made the next apartment complete in everything that can conduce to the comfort of the Fellows who visit it, either for reading, writing, or consultation; and, situated as we are, at the very centre of our principal thoroughfare, they may, I think, be disappointed that so few of our members appear as yet to avail themselves of the accommodation afforded them. But they have taken a much more important step than this; they have devoted three hundred pounds, not from the capital, but from the savings of the Society, to the increase of the Library. Every department of science has been fairly represented in the sub-committee appointed to expend this sum; and if the Naturalists have carried off the lion's share in the distribution, this has arisen from no unfair preference, but from the great expensiveness of their necessary apparatus. A tolerably extensive library of mathematics or philology may be purchased at the price of a single publication on shells or ferns. In the geographical department our collection is eminently rich. We possess, and have mounted in a new and very serviceable manner, maps to the amount of 625 sheets. Of these 439 relate to Europe, and 78 to Asia; and many are from elaborate surveys, and on a large scale. The Council has also been busily employed during a great portion of the year in preparing a Catalogue of the Library. The completion of this has unavoidably been impeded by the gradual accession of additional books; but it is hoped that in the course of the present session, or of the succeeding summer, a complete and well-arranged Catalogue will be accessible to the Members of the Society; and that from it the students of every branch of science will learn that valuable contributions to their favourite branch, whatever it may be, have been recently made, with an especial view to the supply of works of reference, which were not to be found in the great public libraries of this city. It may not be uninteresting to the literary members to know that a considerable number of Dictionaries, Grammars, and works on general Philology, are among the recent additions to the library.

Another important subject to which the attention of the Council has been directed, is the finance of the Society. Several circumstances have shown it to be desirable that the amount both of admission and annual payments should be diminished; and the statements drawn out by our very intelligent and zealous Treasurer, show that such diminution may be made without incapacitating the Society from carrying out any of its legitimate purposes. As the rate of fees is fixed by a law of the Society, the sanction of a general meeting will be necessary to the alteration; and a motion to that purpose will, I believe, be made this evening.

In recounting the duties of the Society, I ought to remind you that we are trustees of three funds devoted to the promotion of science; and are the judges appointed to select among competing candidates those most deserving of the prizes afforded from the interest of these funds. The first of these prizes is the Keith Gold Medal and Prize, given biennially to the author of that paper read before the Society which the Council considers as the most valuable contribution to science, made through the Society, during the two preceding sessions. The second is the Brisbane Prize, the special application of which was left by the learned and liberal donor entirely to the judgment of the Council. They have decided that this prize shall be awarded, at biennial periods alternating with the Keith Prize, and that for the first biennium it shall be awarded to the author of the best Biographical Memoir of some deceased Scotchman, distinguished by his scientific attainments. Thirdly, we have the Neill Bequest, which, in conformity with the well-known pursuits of the founder, will be devoted to the encouragement of natural history in its various branches. We are thus empowered to invite and stimulate and reward exertion—1*st*, In the great field of physical and experimental science; 2*d*, In mathematics and astronomy; 3*d*, In the investigation of the forms, properties, and relations of the various families of the organized creation. The destination of the Brisbane Prize appears to have this peculiar merit, that it gives scope for the exhibition of literary as well as of scientific merit; and I hope that those who may be induced to compete for it will remember that each of the heroes of science was not an abstract intellect, but a man, with human affections and passions acting for good or for evil—with moral and religious tendencies, influencing, it may be, his scientific pursuits, and colouring his enunciation of his discoveries. Good biography, the accurate life-like portraiture of a great mind, is one of the highest achievements of literary skill.

Having thus directed your attention to some of the secondary offices and purposes of the Royal Society, I feel it right to revert to that which the external world will always consider as its primary duty, and by the adequate performance of which, without reference to anything else, the Society must rise or fall in the estimation of men of science at home and abroad; I mean, of course, the annual publication of a valuable fasciculus of Transactions. To this point the efforts of the leading members of the Society ought to be especially directed. Individually they may have scientific reputations to make: but they have not to create, but to maintain, the reputation of the Society. The papers contributed to our Transactions by Robison, Ivory, Hutton, Playfair, Hope, and Hall, will bear comparison with any on like subjects, and of the same date; while, to mention only one of our living members, the optical papers of Sir David Brewster have carried the name of the Royal Society of Edinburgh, in conjunction with his own, through the whole of the scientific world.

But though we may be obliged to confess that our more recent publications are inferior to those of an earlier date, this is not to be attributed entirely, if at all, to a falling off in industry or talent among our members. In the first place, there are ebbings and flowings in all intellectual pursuits; and I am told by those who know more of the matter than I do, that at present the tide of science is not flowing either here or elsewhere. Such turns of the tide in an advancing direction are, I think, generally attributable to the rise of some man of genius who gathers round him, and stimulates and directs the minds of those whose talents are of kin to his own genius. In this leading class we may place such men as Linnæus, Laplace, and our own Sir Humphry Davy; and I feel sure that Cambridge has lived and acted for a century and a half, not upon the reputation, but under the abiding influence of Newton and Bentley. If, then, science be at present but slowly progressive, it is, I suppose, because the men of talent, of whom there is no lack, are in want of a man of genius to lead them on.

Whatever may be thought of this, there exist causes which render the preparation of a good volume of Transactions more difficult in the present day than at any former period; and these difficulties are not peculiar to our Society, but are felt, I believe, by all similar institutions. The first of these is the multiplication of scientific societies, each devoted to some particular branch—Chemical, Astronomical, Geological, and Botanical. Whether science is more effectively promoted by such specific associations, or by those which, like our own, give a general admission to contributions in every branch of science, I do not take upon me to say. The practice of all the civilized nations in maintaining, under some designation or other, an academy of science, and giving to it a pre-eminence above societies working in a limited field, shows I think a general feeling that the necessity for the former is not superseded by the multiplication of the latter. And there are reasonable grounds for this feeling. The dictum of the orator in accounting for his interest in the poet is so universally admitted as almost to have passed into a proverb: "Etenim omnes artes quæ ad humanitatem pertinent, habent quoddam commune vinculum, et quasi cognatione quadam inter se continentur." To strengthen this vinculum and relationship is not the least important office of the Royal Society: and therefore whatever attraction our members may find in societies instituted for the exclusive promotion of their own favourite pursuits, they will, I trust, never abandon their allegiance to science in the largest acceptation of the term, nor their co-operation with that society which gives a cordial reception to every art, *uæ ad humanitatem pertinet.*

Still I fear that such specific societies, whether publishing their own transactions, or sending them to the various specific journals, must draw away many valuable papers, which at an earlier period would have found no convenient channel of publication but in the pages of our Transactions. This turning of our supplies into other channels it is impossible for us to prevent; and so that science is promoted, we ought not to care very deeply whether this is done through us or through others. But a generous emulation is something very different from an envious rivalry; and the activity and success of other scientific societies ought to stimulate those of our brethren who have already proved their competence, to continued and increased exertions to promote the usefulness and the reputation of the Society.

And now I must refer to a subject, which, indeed, if we be a Society actuated by a really social spirit, must be more or less in the minds of the Fellows, when we meet for the first time after the interval of the summer vacation. The interval is not long, and yet it has been sufficient to produce great changes in the managing portion of the Society, by the removal from this world of several of those who united high scientific attainments with a deep interest and careful attention to the ordinary business of the Society. The members removed by death during the last year are Sir George Ballingall, Professor Gray, Colonel Madden, Mr John Clerk Maxwell, General Martin White, and Mr James Wilson.

It might be thought invidious for the Society acting in its corporate capacity to single out among these some whose memory deserved commemoration above that of others. But an individual can speak of that only which he himself knows, and must be allowed to speak in preference of those whom he knew, not merely by the reputation of their talents, but more closely in the intercourse of professional or of social life.

I mention then, in the first place, Sir George Ballingall, who closed a life of much useful activity at the advanced age of seventy. His scientific labours, so far as I am informed, were very much limited to his profession; his more important works, such as his Lectures on "Military Surgery," "On the Construction of Hospitals," "On the Diseases of India," were all intended to communicate to the younger members of the medical profession the results of his own long and careful experience as an army surgeon and as a medical officer in India; and his more numerous occasional papers, having all the same professional character and purpose, appeared, not in our Transactions, but in the journals of Medical Science. Throughout his long career in the army, in the University of Edinburgh, and in private practice, Sir George possessed the confidence and esteem of all with whom he was connected, and this was due not only to his professional knowledge and skill, but also to his upright and gentlemanly deportment in private life.

I have next to speak of one whose name and person at least have been more under the notice of the younger Fellows of the Society, from the circumstance of his having very kindly and very effectively undertaken the duties of General Secretary, when for a time we were under great difficulty from the severe and protracted illness of Professor Forbes. I should not have ventured upon any attempt to delineate, even in outline, the moral and scientific character of Mr James Wilson, had I not felt that whatever might be thought or said of him elsewhere, something was due to his memory in this place and at this time. I should not have attempted it, because the branches of science which he cultivated have never occupied my attention, and because the whole of his character, both in its moral and intellectual aspects, has already been depicted by our brother Professor George Wilson, who knew his honoured namesake more intimately than I did, and who is far better qualified than I am to speak of his scientific labours. Indeed, I must confess that, whatever, in the execution of the office assigned me, I feel bound to say respecting our excellent and lamented brother, James Wilson, is either borrowed from or confirmed by the beautiful Memoir of him which has appeared in the Edinburgh New Philosophical Journal, from the pen of Dr George Wilson—not a kinsman, I believe, but certainly a man of kindred spirit with the subject of his Memoir.

A weakness of constitution, which manifested itself in his early manhood, withdrew Mr Wilson from the labours of a profession; and as his leisure permitted, so his inclination prompted him to devote himself to the study of animated nature. His retiring modesty could not prevent his becoming known as an accomplished naturalist; "and after the death of the late Professor Edward Forbes, the Chair of Natural History in the University of Edinburgh was offered to, but declined by, Mr Wilson. He was an acknowledged authority on Entomology, and scarcely less distinguished as an Ornithologist and Ichthyologist." His published contributions to science, generally anonymous, were extensive and important. To the seventh edition of *The Encyclopædia Britannica* alone he furnished a whole volume of articles, amounting to 649 pages, all on subjects of Natural History. He contributed largely to the various scientific journals, and to the transactions of scientific societies, while at the same time his literary talent and genial humour were shown by many interesting papers which appeared in the more popular Magazines and Reviews of his time.

But Mr Wilson has a higher claim on our affectionate remembrance than could be founded upon his scientific labours alone. He was a good man of a high type of goodness. The gentleness of his temper must have been apparent to all who had any intercourse with him in the business of this Society; but those who knew him most intimately concur in testifying that his naturally amiable mind was indebted for much of its charm to the pervading influence of a deep religious principle; that he sought after God, not in his works only, but in his word also; and that he closed his blameless and useful life by a death robbed of its sting, and left this world with a humble reliance upon the promise of better things to come.

We have lost another and very kindred spirit by the death of Colonel Edward Madden. Him I knew intimately, and though his favourite track of science was very remote from my pursuits, I soon learned that his mind had many sides and could not fail to interest any one who had a respect for talent or a love for goodness. Colonel Madden joined our Society in 1853, and not having, as far as I know, read any papers at our meetings, he was probably little known to a large portion of the Fellows. But his character and attainments were well known to botanists, and they gave a sufficient proof of the estimation in which they held him by electing him to the Presidency of the Botanical Society. He was, soon after his admission as a Fellow, elected into the Council of this Society, and rendered valuable assistance as a member of the Library Committee, from his extensive knowledge, not of his own science only, but also of the apparatus required by the student of geography and of philology.

I have to express my gratitude to Dr Falconer of London for having supplied me with notices respecting Colonel Madden's pursuit of Science in India, much beyond what I can use on the present occasion, and which I shall return to him, in the hope that he may employ them in raising a worthy memorial of our departed friend.

From these notices it appears that before Colonel Madden's attention had been directed to the vegetable kingdom, and when he was a lieutenant of artillery in the Company's service, he employed a leave of absence in search of health among the lower ridges of the Himalaya. Health he found; but he found something more,—his own proper vocation as a lover and a student of nature. In no other region, probably, could his natural powers and tendencies have been so strongly called into action. No region presents the leading phenomena of physical geography in greater contrast, both as regards the varieties of human races, difference of vegetable and animal life, meteorological and other climatic conditions, than the north-western plains of India, and the stupendous chain of mountains by which they are bounded on the north. Here, in comparative proximity, are found the vegetable productions of the Torrid and the Temperate Zones, while the traveller, as he ascends through a belt of Alpine character, reaches at length the region of perpetual snow.

When Lieutenant Madden first visited these interesting regions, he appears to have been totally unacquainted with systematic botany. But he brought with him a vivid recollection of the vegetable forms which he had noticed at home, and a tendency and capacity for observing every affinity or contrast to these in the objects which surrounded him in India. These observations were regularly noted down; and though necessarily very imperfect, they were of material service to him when afterwards he prepared and published his memoirs "On the Plants of the Turaee," and "On the Coniferæ of the Himalaya."

A few years after this tour, Colonel Madden revisited the Himalaya, and was there fortunate in making the acquaintance of Dr Falconer, at that time Superintendent of the Botanic Garden at Saharunpore, a station very near the foot of the Himalaya. It was here that his mind was formed to the systematic study of his favourite science. Here he had access to a rich collection of plants, to a well-stored herbarium, to a good botanical library, and to the society of experienced and friendly instructors. Of these advantages he made the most; and the fruits of his studies were shown in his first publication of any importance, entitled, "Brief Observations on Himalayan Coniferæ." This was first published in an obscure local journal, but reprinted in the "Journal of the Agricultural Society of Calcutta," and through that channel found its way into general notice among the botanists of Europe. A supplement to this paper, more extensive than it, was printed in the latter journal in 1850, after the author had left India. These memoirs are of such striking merit that they were transferred *in extenso* by Dr Lindley into the Journal of the Horticultural Society of London.

Soon after leaving Saharunpore, Colonel Madden was removed to a station in the hill province of Kumaoon. He was there fortunately brought into co-operation with the two brothers, Captains

Henry and Richard Strachey, at that time employed upon an inquiry into the physical geography of that and the adjoining Hill Provinces. The results of the labours of the Stracheys are well known, through memoirs communicated to the Royal Geographical and Geological Societies. Colonel Madden was an active colleague to Captain Richard Strachey in the botanical branch of this survey; and in 1848 he published the results of his observations in a very valuable memoir on "The Turaee and outer mountains of Kumaoon," which appeared in the Journal of the Asiatic Society of Bengal. These are spoken of by Dr Falconer as models of careful observation on the geographical distribution of plants, and at the same time as rich in illustrations, drawn from every department of a well-stored mind, and a wide and varied range of literature.

Hitherto, I have been speaking of Colonel Madden from the notes of his attached friend Dr Falconer, and in reference to scientific attainments and labours on which, you are all aware, that I am incompetent to form, and *à fortiori*, to express an opinion. All of you who knew him in social life, or in the Council meetings of the Royal Society, must remember with affectionate regret the gentleness of his manner, and the unobtrusive modesty with which he gave his assistance only when it was needed, and where he was sure of the precision of his knowledge. In respect to the highest wisdom, it appears that from his youth he was actuated by that love of the true and the good which constitutes the character of those who, if not actually in, are at any rate not far from the kingdom of Heaven. Careful and conscientious inquiry led him from doubt to conviction; and his latter years were spent under the influence of an assured faith and a steady resolution to do the will of God.

Such were some of those who have been taken from among us in the course of the last year, and whose virtues and useful labours will not be forgotten by those with whom they co-operated for the advancement of science.

I must now conclude my very imperfect performance of the task imposed upon me by the Council, with the expression of a hope that, on future occasions of the same kind, they may be more fortunate in their choice, and obtain addresses more worthy of occupying the time and attention of the Society.

C. H. T.

The following statement as to the Members of the Society was read by the Chairman:—

Ordinary Fellows at November 1855,		267
Add one name omitted by mistake,		1
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Deduct deceased—Sir G. Ballingall, Professor Gray, Colonel Madden, Mr John Clerk Maxwell, Dr Wilson Philip,* General Martin White, Mr James Wilson,		7
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Resigned—Mr Forbes of Culloden, Mr Grant of Elchies,	2	
Struck off for non-payment of Admission Fees—Mr E. Bonar,	1	3
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But add new Fellows—Dr Allman, Mr Bryce, Mr Cleghorn, Mr Mitchell Ellis, Mr James Hay, Dr Laycock, Professor Clerk Maxwell, Lord Neaves, Dr Penny, Mr R. M. Smith,		10
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The following Communication was read:—

On the Minute Structure of the Involuntary Muscular Tissue. By Joseph Lister, Esq., F.R.C.S. Eng. and Edin. Communicated by Dr Christison.

In this paper the author, after a short general account of the different forms in which contractile tissue occurs in the human body, describes at greater length the discovery made in 1847 by Professor Kölliker, that involuntary muscular fibre is capable of being resolved into nucleated elements, supposed to be of the nature of elongated cells, and hence termed "contractile" or "muscular fibre-cells." He then alludes to some authorities who object to this view of the structure of involuntary muscle, and notices, especially, a paper by Professor Ellis of University College, London, read before the Royal Society of London in June of the present year (1856), in which that distinguished anatomist expresses his belief that "the fibres are long, slender, rounded cords of uniform width," and that the nuclei "appear to belong to the sheath of the fibre;" whence it is to be inferred that Kölliker's fibre-cells are, in the opinion of Mr Ellis, created by the tearing of the tissue in the preparation of the objects.

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* Dr Philip has been for some years dead.

The author then proceeds to describe the involuntary muscular tissue as it presents itself in two situations where he has recently examined it, namely, the minute arteries of the frog's foot and the small intestine of the pig. He finds that, by suitable manipulation, exceedingly delicate arteries may be dissected out from the web of the frog, some of them being of smaller calibre than average capillaries; and that in such vessels the middle coat consists of neither more nor less than a single layer of Kölliker's muscular fibre-cells wrapped spirally round the internal membrane, and of sufficient length to encircle it from about one and a half to two and a half times. The tubular form of the vessels enables the observer, by proper adjustment of the focus, to see the fibre-cells in section; and where the nucleus is so placed in the artery as to appear in section also, the section of the nucleus is invariably found surrounded on all sides by that of the fibre-cell, whence it is inferred that the nucleus is not merely connected with the external part of the muscular element, but is embedded in its substance. Considering that no tearing of the tissue is practised in the preparation of the objects, but that the parts are seen undisturbed in their natural relations, this simple observation appears to prove conclusively, that, in the arteries of the frog's foot, the involuntary muscular tissue is constituted as Kölliker has described it.

The pig's intestine proved to be a very favourable situation for the investigation of unstriped muscle, the fibre-cells being larger than in the human subject in the same situation, and very readily isolated by simply teasing out a small portion of the tissue with needles in a drop of water. Under these circumstances, they corresponded exactly with Kölliker's descriptions, and the delicate and perfect form of their tapering extremities was sometimes seen to be such as could not possibly have been produced by the tearing of a continuous fibre. In one fibre-cell that happened to be coiled up, the position of the nucleus embedded in its substance was seen in the same way as in the arteries of the frog. In examining the circular coat of a contracted piece of intestine from a freshly killed pig, the author observed some short, substantial-looking bodies of high refractive power, each of a somewhat oval shape, with more or less pointed extremities, and presenting several strongly-marked, thick, transverse ridges upon its surface; and each, without exception, possessing a roundish nucleus, whose longer diameter lay across that of the containing mass. Between these bodies and the long and delicate fibre-cells every possible gradation could be traced, and it was therefore pretty clear that the former were but the extremely contracted form of the latter. That the appearances in question were due to contraction of the fibre-cells, was proved by their disappearance when a portion of the tissue was strongly stretched.

The bearings of these observations on the main question, respecting the structure of involuntary muscular fibre, are obvious and important. In the first place, if the short substantial bodies were mere contracted fragments of rounded fibres of uniform width, we should expect them to be as thick at their extremities as at the middle; instead of which they are always more or less tapering, and often present a very regular appearance of two cones applied to each other by their bases.

Secondly, the uniform central position of the nuclei in the contracted fibres, proves clearly that the former are no accidental appendages of the latter, to which it seems difficult to refuse Kölliker's appellation of *cells.*

In conclusion, the author makes the following remarks :—

To sum up the general results to which we are led by the facts above mentioned, it appears that in the arteries of the frog, and in the intestine of the pig, the involuntary muscular tissue is composed of slightly flattened, elongated elements, with tapering extremities, each provided at its central and thickest part with a single cylindrical nucleus imbedded in its substance.

Professor Kölliker's account of the tissue being thus completely confirmed in these two instances, and the description here given of its appearance in the arteries of the frog's foot being an independent confirmation of the general doctrine, there seems no reason any longer to doubt its truth.

It further appears, from what has been seen in the pig's intestine, that the muscular elements are, on the one hand, capable of an extraordinary degree of extension, and, on the other hand, are endowed with a marvellous faculty of contraction, by which they may be reduced from the condition of very long fibres to that of almost globular masses. In the extended state they have a soft, delicate, and, usually, homogeneous aspect, which becomes altered during contraction by the supervention of highly refracting transverse ribs, which grow thicker and more approximated as the process advances. Meanwhile the "rod-shaped" nucleus appears to be pinched up by the contracting fibre, till it assumes a slightly oval form, with the longer diameter transversely placed.

The author further remarks, that these properties of the constituent elements of involuntary muscular fibre explain in a very beautiful manner the extraordinary range of contractility which characterises the hollow viscera.

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Beskrivelse over Skotlands Almueskolevæsen tilligemed forslag til forskjellige foramstalt-ninger til en videre Udvikling of det norske Almueskolevæsen. Af Hartvig Nissen. 8vo.—*From the Author.*

Beretning om fante-eller Landstrygerfolket i Norge. Af Cilert Sundt. 8vo.—*From the Author.*

Beretninger om Sygdomsforholdene i 1842 og 1843 i Danmark, Sverige og Norge, oplæste ved de Skandinaviske Naturforskeres Möde i Christiania 1844. 8vo.—*From the University of Christiania.*

Konge—Speilet et Philosophisk-Didactisk Skrift, forfattet i Norge mod Slutningen af det tolfte aarhundrede. 8vo.—*From the same University.*

Aslak Bolts Jordebog. Fortegnelse over Jordegods og Andre Herligheder tilhörende erkebiskopsstolen i nidaros, affattet ved erkebiskop aslak bolts Foranstaltning mellem aarene 1432 og 1449. Af P. A. Munch.—*From the same University.*

Kong Olaf Tryggvesöns Saga forfattet paa latin henimod slutningen af det tolfte aarhundrede af Odd Snorreson. Af P. A. Munch. 8vo.—*From the same University.*

Lycidas ecloga et Musæ Invocatio, carmina quorum auctori Johanni van Leeuwen, e vico zegwaart certaminis poetici præmium secundum e legato Jacobi Henrici Hœufft adiudicatum est in concessu publico Academiæ Regiæ Scientiarum, die 13 Maji, anni 1856. 8vo.—*From the Author.*

Diem Natalem Augustissimi Regis Caroli Joannis ab Universitate

Regia Fredericiana, die 26 Januarii 1838 celebrandum indicit Collegium Academicum. 4to.—*From the University.*

Dr Lorenz Hübner's Biographische Charakteristik. Von Joseph Wiskmayr. 4to.—*From the Royal Academy, Munich.*

Ueber die Gliederung der Bevolkerung des Königreichs Bayern. Von Fr. B. W. Von Hermann. 4to.—*From the same.*

Maps of the Geological Survey of India.—*From the Hon. East India Company.*

Monday, 15*th December* 1856.

PROFESSOR CHRISTISON, V.P., in the Chair.

The following Communications were read:—

1. On the Ovum and Young Fish of the Salmonidæ. By William Ayrton, Esq. Communicated by Professor Allman.

The paper contained a series of observations on the development of the embryo in the salmon, made on ova procured from an establishment for the artificial propagation of this fish, at Overton, on the river Dee. The author's observations commenced about the 37th day after impregnation, and were continued from that period to the time when the vitellus becomes finally absorbed. The progressive development of the various organs was described, and the principal steps illustrated by carefully drawn and expressive figures. With regard to the proper time for transit, the author arrived at some practical conclusions. He maintained that the ova, after impregnation, should be as little disturbed as possible for the first thirty or thirty-five days, but he proved that at the end of that period they may be exposed to great changes of temperature, and of other external conditions, with comparative impunity; and he was of opinion that they would then endure a transit of some days if only supplied with moisture by means of moss, wool, or similar material. He further concluded, as the result of various observations and experiments, that if the transit be not made when the ovum is from thirty to forty days old, it will be made with most safety after the young fish has attained an age at which the yelk is wholly or nearly absorbed.

2. Notice of the Vendace of Derwentwater, Cumberland, in a letter addressed to Sir William Jardine, Bart., by John Davy, M.D.

In this communication the author first gives an account of the occurrence of the Vendace (a fish hitherto supposed to be confined to the lochs of Lochmaben in Dumfries-shire) in two of the lakes of Cumberland, viz., Derwentwater and Bassenthwaite Lake, in each of which it seems to be pretty abundant, and to have been long known to the boatmen; secondly, he offers some speculative suggestions as to the diffusion of species in the instance of this fish, giving the preference to that presuming that its transfer might have been accidentally effected by the impregnated ova being conveyed by aquatic animals: adducing in favour of this conjecture facts ascertained by him respecting the ova of the Salmonidæ, such as their bearing exposure in a moist atmosphere for days without losing their vitality, and also, with equal impunity, the reduction of their temperature to the freezing point of water, and their entanglement in and adhesion to ice.

3. On the Races of the Western Coast of Africa. By Colonel Luke Smyth O'Connor, C.B., Governor of the Gambia. Communicated by Professor Kelland.

The British possess three colonies on the western coast of Africa: Gambia, 2490 miles from England, in latitude 13° 30′ N., longitude 14° 40′ W.; 500 miles south, Sierra Leone, in latitude 8° 30′ N., longitude 13° 10′ W.; and, 1500 miles down in the Bight of Benin, Gold or Cape Coast Castle, in latitude 5° 5′ 25″ N., longitude 1° 12′ 45″W.

Gambia is selected for this paper, as not only the first, but the most singular and interesting of our African colonies.

Nearly two centuries have glided away since the British, by permission of the kings and chiefs of Combo, established a settlement on the island of St Mary's,—a mere bank of sand, swamp, mud, and mangroves, pregnant with miasma, and well stocked with alligators,—situated near the mouth of the river Gambia, along whose banks we by degrees established small trading ports or factories, until at last we reached Pisania, nearly 300 miles up the river, where Dr Laidley superintended an extensive trading depôt in 1786, and for years continued the able head and manager of it. But singular enough, with all our wealth, enterprise, desire to extend commerce, and procure channels for the circulation of our home manufactures, we are to the present moment ignorant of the *source and course* of the Gambia; having but hazy, legendary records of the wild tribes adjacent to its banks, or of the children of the desert, the "Foutah Foulahs" and "Foutah Toros," who, rushing down periodically from the interior, devastate districts, plunder whole villages, and bear off into slavery men, women, and children. Our settlements in the Gambia are surrounded by warlike and powerful nations, the kings of Barra, Combo, Badaboo, Kattabar, Woollie being the chief.

The kings of Barra long ruled with despotic sway all the minor sovereigns, and, as their dominions lay adjacent to ours, and extended for several miles along the right bank of the river, we had to conciliate their favour, and submit to their unjust, tyrannical, and insatiable exactions, to preserve intact our struggling settlements of Bathurst, and secure the lives and properties of our merchants trading in the river to Pisania.

The head man of the kings of Barra was, and indeed still is, the "Alkadee of Jillifree," from time immemorial the most powerful chief in the Gambia among the adjacent nations and with the distant tribes. These kings are mentioned by several travellers in Western Africa,—by Johnson in 1621, Stibbs in 1723, Moore in 1737, Dr Laidley, the special friend of the gallant Houghton, in 1791, the enterprising Mungo Park in 1795, and again, when he left Pisania, upon his last fatal expedition to the Niger, in May 1805, by Snelgrove, Winterbottom, Meredith, Houghton, Murray, and many other writers,—as great chiefs, who held the principal sway in the kingdom of Barra. They collected the king's dues (for in former times the kings of Barra levied a duty of £18 on all vessels proceeding up the river Gambia beyond Jillifree), and monopolized the salt trade as far as the kingdom of Woollie; in consideration of which, the kings engaged to redress the wrongs of all traders, factors, agents, or caboceers, who held commerce with them; and did so effectually, by sending a war-man and canoe to any bey, chief, or "sooma," who dared to molest friends belonging to them, that is, under their immediate care.

Two hundred miles up the river Gambia is our most distant military station, M'Carthy's Island, which we purchased from the king of Kattabar. It is about nine miles in length, and one to one and a half in breadth, and forms the principal depôt for the merchants' goods, consisting of blue and white bafts, cotton prints, sugar, guns, gunpowder, tobacco, salt, and rum—the white man's fire water, which has been and is the burning curse—the poisonous draught—the fatal fountain from whence flows the long list of crimes which stain the wild Indian's and the savage African's career for centuries. To it may be traced the bloodiest records in the history of the Old and New World; and the moral, religious, civilized Europeans have a fearful account to answer for.

Beyond M'Carthy's Island the river becomes wilder, more romantic, the banks loftier and more thinly inhabited. Alligators, hippopotami, baboons and monkeys, of great size, and uncommon strength and ferocity, crowd the banks, and follow the steamer or boats with barking and howling, little intimidated by the discharge of fire-arms. The natives entertain a respect, mingled with fear, for the large baboons, and class them with the devil. Indeed, when passing a hill, said to be the especial resort of the devil, the natives salute, not by profound bows, genuflexions, retreating backwards, or flinging dust upon their heads, but by turning their backs, and dancing an antic bolero for some minutes; and they are so firmly persuaded of the necessity of this ceremony, that no promise or reward will induce one of them to pass the *Devil's Hill sans* salutation.

The far-famed Falls of Barraconda, beyond which few white men have advanced, are nothing more or less than shoals, formed by the river rushing over a ledge of sunken rocks, extending diagonally across the channel. In the rains, a vessel can pass over the Falls, and the whole country is inundated for miles.

A few hunters for ivory, or wandering traders, occasionally frequent the Falls, but no factories or settlements extend to this distance. Captain Stibbs, who was despatched by the Duke of Chandos, Director of the Royal African Company, in 1723, proceeded beyond the Falls of Barraconda, until the river became too shallow to float the boats, even when the channel was 160 yards broad. He describes the few natives he met with as a harmless people, who supplied him abundantly with fowls and provisions; but he found himself in the region of elephants, river-horses, and baboons. Nearly a century and a half have passed since Stibbs explored the river, and we are not better acquainted with the region of "river-horses" nor of the nature, customs, manners, and habits of the harmless dealers in "fowls and provisions."

The nations not only adjacent to our settlements on the river Gambia, but as far as we carry on commerce, may be classed under the heads of *Mandingoes and Jolliffs*, although divided and subdivided into several tribes bearing different names, such as Warasoonkos, Labous, Surruwoollies, Syreras, Teboos, Tuarics, Jolahs; the last having hardly the features and semblance of human beings. The Mandingo language is soft, musical, poetical, and might be styled the Italian of Western Africa. The Jolliff is harsh, guttural, and confined. Upwards of one hundred dialects are to be met with.

The Arabic character is used for the Koran, general despatches, public or private communications; and the Marabouts instruct the children in the different villages by means of large oval pieces of board, on which letters and sentences from the Koran, or the "Prophet's Laws," are written with a pen made from the common reed. Frequently in my ramblings I have come at night-fall to a Mohammedan town, and after being introduced to my "lodging-man," or landlord, who is bound to protect both my person and my purse, strolling through the narrow closely-fenced paths—for streets they were not—I have reached a small but open space in front of a circular hut, at the door of which was seated a venerable, white-bearded Marabout, surrounded by a crowd of children of all ages, each holding his *wooden* book, and repeating in full chorus, an extract from the Koran, given forth in a clear sonorous voice by the aged instructor.

Polygamy and slavery are the great banes of Western, if not of all Africa. The former checks population, shatters all domestic ties, all kind and friendly feeling; reduces woman to the lowest, most degraded position, so that she becomes the beast of burden, the servant of servants of one tyrannical master.

Slow and uncertain has been our progress in the river Gambia. Since the days of Johnson, in 1621, or Dr Laidley, 160 years afterwards, or Mungo Park, in 1805, to my two trivial expeditions in 1854 and 1856, we are still ignorant of the source of the Gambia; whether (as is vaguely surmised) it connects itself with the Senegal, or flowing southward into the *Niger*, and joining its volume of waters to those of that singularly obscure and interesting river, empties them, thousands of miles from our settlement at Bathurst, into the Bight of Benin.

The following Donations to the Library were announced:—

The Canadian Journal. New Series. No. 6. 8vo.—*From the Canadian Institute.*

Silliman's American Journal. No. 66. 8vo.—*From the Editors.*

Journal of the Statistical Society of London. Vol. XIX., Part 4. 8vo.—*From the Society.*

Schriften der Universität zu Kiel, 1854, 1855. 4to.—*From the University.*

Bulletin de la Société de Geographie. 4^{ieme} Serie, Tom. XI.—*From the Society.*

Abhandlungen herausgegeben von der Senckenbergischen Naturforschenden Gesellschaft. Band II., 1ste lieferung. 4to.—*From the Society.*

Monday, 5th *January* 1857.

Professor CHRISTISON, V.P., in the Chair.

The following Communications were read:—

2. Some Remarks on the Literature and Philosophy of the Chinese. By the Rev. Dr Robert Lee.

2. Observations on the Crinoidea, showing their connection with other branches of the Echinodermata. By Fort-Major Thomas Austin, F.G.S. Communicated by Professor Balfour.

The author remarks, that although there are upwards of 280 writers on the Crinoidea, yet there is no class of ancient animals so much misunderstood. These animals have not merely played an important part in the system of creation as regards animal life, but they have also modified the physical condition of the globe. They are found abundantly in limestones, both of the Silurian and of the Carboniferous epochs, attaining their maximum in the latter. The author traces the Crinoids, or Encrinites, through various strata, showing the gradual disappearance of ancient forms, and their replacement by new forms of Radiata. The chief characteristics of the Crinoidea, up to the Carboniferous epoch, are, that the articulations which connect the indurated pieces of which the column is composed radiate by simple striæ diverging from the central axis; and that the dorsal portion of the body, that is, the part below the rays, generally preponderates over, or at least is fully equal to, the ventral or upper portion. From this period these older types gradually became extinct, and we find them succeeded by forms in which the ventral portion is generally superior in size to the dorsal, which seems now to serve only as a base for the support of the wide-spreading rays; while, with two exceptions (the Apiocrinus and Gnathocrinus), the articulations in the columns are secured by crenulated floriform ridges on the facets of the joints. This peculiar form attained its maximum in the Lias; since which it has dwindled down to a solitary Pentacrinus, and a few other Crinoids, having little resemblance to the ancient forms.

The author then gives a historical sketch of the various opinions entertained regarding these animals, and notices the superstitious ideas regarding them, whence had originated the names given to them of fairy-stones, giants' tears, St Cuthbert's beads, star-stones, wheel-stones, screw-stones, &c. After alluding to the views of early authors, he proceeds to show the advances which have been made of late in our knowledge of the structure and affinities of Crinoids. He traces the analogies between them and the Echinodermatous tribe. From the Comatula we pass through a succession of forms onwards to the most perfect Echinoderm (the sea-urchin), and backwards to the Marsupite, the Pentacrinus, and the Crinoidea. General affinities exist between the free swimming Euryale and the Marsupite, as well as the fixed Euryalicrinus of the Silurian and Carboniferous rocks.

The analogy between fossil Crinoids and the recent forms of Echinoidea is noticed. The author traces the gradual and uninterrupted transition from the Asteriadæ to the Crinoidea, from the Crinoidea to the Blastoidea, from the latter to Echinida, and finally to the star-fishes.

The following Gentlemen were admitted as Ordinary Fellows:—

Horatio Ross, Esq.
James Black, M.D., Lic. R. Coll. Phys. Lond. F.G.S.

The following Donations to the Library were announced:—

An Analysis of the Statistics of the Clearing House during the year 1839; with an Appendix on the London and New York Clearing Houses, and on the London Railway Clearing House. By Charles Babbage, F.R.S. 8vo.—*From the Author.*

Sanatory Remarks in connection with Nuisances. By Thomas Williamson, M.D. 8vo.—*From the Author.*

Monday, 19*th January* 1857.

Right Reverend BISHOP TERROT in the Chair.

The following Communications were read:—

1. On the application of the Theory of Probabilities to the question of the Combination of Testimonies. By Professor Boole. Communicated by Bishop Terrot.

The method for the solution of questions in the theory of probabilities which is applied in this paper, is that which was developed by the author in a treatise entitled "An Investigation of the Laws of Thought, on which are founded the Mathematical Theories of Logic and of Probabilities."

The special problems with relation to the combination of testimonies to which the method is applied are the following, viz.:—1*st*, That in which the testimonies to be combined are merely different numerical measures of a physical magnitude, as of the elevation of a star, furnished by different observations taken simultaneously. 2*d*, That in which the testimonies to be combined relate, not to a numerical measure, but to some fact or hypothesis of which it is required to determine the probability; the probabilities furnished by the separate testimonies being given.

Before proceeding to the solution of these problems, the author notices a distinction between problems (relating to probabilities) of which the elements are logical, and problems of which the elements cannot, without being subjected to some previous mental transformation, be regarded as logical. He describes as logical those problems in which the data are the probabilities of certain simple events, and the quæsitum the probability of some compound event, whereof the simple events are the elements combined in any way as happening or not happening. If, for example, the probabilities that the individuals A, B, C, will continue in life for ten years, be p, q, r, given fractions, and the quæsitum be the probability of all the lives continuing, or of their all failing, or of any two falling and one continuing; these he calls logical problems. In like manner, if the data be the probabilities of such compound events, and the quæsitum be the probability of some other compound, or of the simple events compounded, these also are logical. This the author expresses generally.

Given prob. $\phi_1(x, y, z)$, prob. $\phi_2(x, y, z)$, &c.; find prob. $\psi(x, y, z)$.

It may be observed, that as an event must either happen or not happen, these functions can consist only of factors of the form x and $1-x$. He observes,—"I regard the elements of a problem relating to probability as logical when the numerical data are the probabilities, in the mathematical sense, *i.e.*, measures of probability of certain events, and when the object sought is the probability of some other event whose connection with the former is either,—1*st*, That it is a compound event of which they are the simple components; or, 2*d*, That both it and they may be resolved into the same components. He employs the term 'compound event' to denote, not merely the conjunction of certain simple events, but any kind of combination of them in the way of happening or not happening, happening together, or in alternation; or in any way which is capable of being expressed by the grammatical forms of language, and especially by the adverb *not* and the conjunctions *and*, *either*, *or*, &c.

According to this definition, the problem of the reduction or combination of different simultaneously-observed altitudes of a star is not in its direct presentation a logical one. The immediate data are not probabilities of events, but measures of a physical magnitude. The author, however, remarks that this problem is capable of being reduced to a form in which its elements are logical. If a quadrant of the celestial arc be taken as the unit of magnitude, then any observed elevation of a star, regarded as a physical point, in a particular quadrant of elevation, will be the probability furnished by that observation, that a pointer directed at random to the given quadrant will point somewhere below the star.

This is clear from the ordinary definition of the measure of probability. For, supposing the observed altitude to be 10°, then the number of different directions that may be given to the pointer, is to the number of such directions falling below the star, as 90 : 10, or as 9 : 1. Hence the probability of the direction of the pointer being below the star at $\frac{1}{9}$ = the altitude of the star, considering the quadrant as unity.

In considering this problem, the author first makes some observations on the different principles which have been employed in its discussion, viz.: the principle of the arithmetical mean regarded as axiomatic in its nature, the principles of mechanical analogy, of geometrical consistency, &c. And he suggests that the agreement of results deduced in such various ways is to be taken as an evidence of a certain harmony between the intellectual powers and the external universe considered as their actual sphere of exercise.

Reducing the problem, as above intimated, to logical elements, he then applies to it the method developed in the " Laws of Thought." The following, though not the most general, is the most interesting result to which the application leads :—

If n observations of the altitude of a star are to be combined, and if c_1, c_2, . . . cn are the several probabilities that these respective observations are absolutely correct, and if p_1, p_2, . . p_n are the altitudes which they furnish, then the most probable altitude will be

$$\frac{\frac{C_1}{1-C_1}p_1+\frac{C_2}{1-C_2}p_1\ldots+\frac{C_n}{1-C_n}p_n}{\frac{C_1}{1-C_1}+\frac{C_2}{1-C_2}\ldots+\frac{C_n}{1-C_n}} \quad (1.)$$

This result is in accordance with the familiar form

$$W_1 p_1 + W_2 p_2 \ldots + W_n p_n$$

where W_1 W_2 . . . W_n represent what are termed the *weights* of the observations. But it is free from any admixture of mechanical analogy, and it expresses the so-called *weights* as functions of certain initial *probabilities*, viz.: the several probabilities of absolute correctness in the observations. Even if these probabilities are, as in theory they must be, infinitesimal, the values of $W_1\ W_2 \ldots W_n$, which depend upon *ratios* will be finite. If any one of the quantities C_1, $C_2 \ldots C_n$, is equal to unity, the formula verifies itself; for if it is certain that if an observation be correct, the result which it furnishes must be adopted.

If $C_1 = 1$, then $\frac{C_1}{1-C_1} = \frac{1}{0}$ = infinity. Therefore, the expression becomes

$$\frac{\frac{C_1}{1-C_1}p}{\frac{C_1}{1-C_1}} = p_1$$

If $C_1\ C_2 \ldots C_n$ are equal, the formula degenerates into

$$\frac{p_1+p_2+\ldots p_n}{n} \ldots\ldots (2.)$$

and expresses the principle of the arithmetical mean.

The author conceives this investigation to be of value, not on account of the result to which it conducts us, but on account of the connexion which it establishes between the doctrine of the arithmetical mean and the logical theory of probabilities.

In the second special problem, viz., the problem of the combination of testimonies to a fact or hypothesis, results are obtained which may thus be described:—

1*st*, The complete solution involves arbitrary constants, and is therefore indefinite. It admits, however, in various cases of a definite value, and leads to many general conclusions of considerable interest.

2*dly*, The arbitrary constants relate, not to the probability of the fact or hypothesis as dependent upon the testimonies or evidences, but to the probabilities of the testimonies themselves. Thus, if two symptoms are observed, each of which gives a certain probability of the existence of a disease, the strength of the joint presumption does not altogether depend upon that of the separate presumption, but is affected, for instance, by the *à priori* likelihood of concurrence of the symptoms.

3*dly*, In general, combined presumptions would be strengthened by the *à priori* unlikelihood of their combination.

4*thly*, In many cases the arbitrary elements disappear. Thus, if one of the presumptions amount to certainty, the combination will always indicate certainty, however unfavourable the opposite presumption may be.

5*thly*, Where testimonies are in a high degree cumulative, *e.g.* where their concurrence was *à priori* very improbable, the formula for the strength of the united testimonies $p_1, p_2, \ldots p_n$, tends to assume the following expression as its limits, viz.:—

$$\frac{p_1 p_2 \ldots p_n}{p_1 p_2 \ldots p_n + (1-p_1)(1-p_2) \ldots (1-p_n)}$$

This is commonly assumed to be the general solution. The author shows that the proof of it, as usually given, involves the neglect of a real and important consideration,—viz., that it is to the *same* fact that the testimonies relate. To the true general solution it stands in the position of a limiting value, applicable only on the hypothesis of the testimonies being of a very unexpected kind, or of their concurrence being very unexpected.

6*thly*, When the probabilities are not cumulative, but some of them are felt to be too great, others too small, and a kind of mean between them is required, a definite result is again obtained, which may be thus stated.

Let $p_1, p_2, \ldots p_n$, represent the separate probabilities, then is the mean probability represented by the formula

$$\frac{(p_1 p_2, \ldots . p_n)^{\frac{1}{n}}}{\left\{p_1 p_2 \ldots p_n\right\}^{\frac{1}{n}} + \left\{(1-p_1)\quad(1-p_2) \ldots (1-p_n)\right\}^{\frac{1}{n}}} \qquad (3.)$$

This formula is the counterpart of (2). It expresses the average of the probabilities furnished by differing judgments, even as the formula of the arithmetical mean expresses the average among differing measures of a numerical magnitude. But it differs in character and in the consequences which it involves from the latter formula.

Thus if the testimonies are two in number, and the probabilities which they furnish in favour of an event are p and q, the formula becomes $\frac{\sqrt{p\,q}}{\sqrt{p\,q} + \sqrt{(1-p)(1-q)}}$, and furnishes a value which does not lie half-way between the values p and q, but which, as may easily be shown, lies nearer to the one of those values which is the nearer either to 1 or to 0. This indicates, that if we have to take an average between two judgments, one of which partakes more of the character of certainty than the other, the former will have greater weight in determining the final state of expectation. This, the author observes, is accordant with our instinctive feelings.

The formula, it is to be observed, is not applicable to cases in which evidence is of a cumulative character, as where different positive assertions are made of the absolute truth of a fact,—but to cases in which different modes of considering a subject lead us to assign different probabilities to a fact or hypothesis, and it is our object to take between these probabilities an average.

For example if A and B, whose veracity, that is the probability of their speaking truth, has a given value, both affirm that the event E has occurred, the formula does not apply. The proper, at any rate the received, formula for such a problem is $\frac{p\,q}{p\,q + \overline{1-p}\,.\,\overline{1-q}}$ But if the question be the probability that Sir Philip Francis was the author of Junius' Letters, and p and q be the probabilities derived from external and internal evidence, then the formula $\frac{\sqrt{p\,q}}{\sqrt{p\,q} + \sqrt{\overline{1-p}\,.\,\overline{1-q}}}$ applies.

In that portion of the memoir, which is introductory to the demonstration of the above results, the author explains the grounds of his method for the solution of questions in the theory of probabilities whose elements are logical. They are briefly the following :—

1*st*, He defines the mathematical probability of an event, as the ratio which the number of cases or hypotheses favourable to that event bears to the whole number of cases conceivable, supposing that to none of those cases the mind is entitled to attach any preference over any other.

2*dly*, He remarks, that when the probabilities of simple events constitute our only data, no knowledge whatever being given of their connection, we can thence, by virtue of the definition, determine the probability of any logical combination of them, either absolutely or conditionally.

3*dly*, He postulates that when the data are not the probabilities of simple events, we can only grasp them and apply them to the calculation of probability by regarding them, not as primary, but as derived from some hypothesis in which the data are the probabilities of simple events, and to which, accordingly, we may apply the principles already referred to, as flowing from the very definition of probability. The probabilities of the simple events in the hypothesis must, of course, be determined in accordance with the original data.

At this stage the question arises, How shall such an hypothesis be lawfully framed? To this question the following answer is given.—

4thly, When, as in the case supposed, the data are not the probabilities of simple events, the numerical measures of probability p, q, r, &c., which they involve, will be subject to certain conditions (beside that of their being positive fractions), in order that these data may be mutually consistent,—may, if considered as furnished by experience, represent an experience which is *possible*. These conditions the author terms the "conditions of possible experience," and he gives a general method for their determination. Thus, for example, if p is the probability of the conjunction of the events x and y, q of the conjunction of y and z, and r of the conjunction of z and x, the quantities p, q and r, besides the condition of not transgressing the limits 0 and 1, must satisfy the conditions

$$p \overset{=}{>} q + r-1,\ q \overset{=}{>} r + p-1,\ r \overset{=}{>} p + q-1;$$

similar conditions deducible from the data, will in general, limit *à priori*, the value of the probability sought.

5thly, The author then lays down the principle, that the hypothetical construction (already referred to) of the problem from simple events with unknown probabilities, must be such, that the determination of these unknown probabilities from the data will be possible and definite, when the above conditions of possible experience are satisfied. In other words, the hypothesis should involve the existence of no other conditions among the data, than the condition of their being possible, and mutually consistent.

This principle, he observes, completely limits and determines the nature of the solution, restricting it to the particular method developed in the chapters on probability in the Laws of Thought. He remarks, that the method in question was not, however, as it first presented itself to his own mind, associated with such considerations as these, nor are such considerations even hinted at in the work referred to. The method was there exhibited as resting upon an axiomatic basis. The fact, that the conditions which it involves as conditions of mathematical validity and consistency, are identical with the conditions of possible experience, was subsequently discovered.

The proof of this identity, is not, however, in its present state, to be considered as complete; neither can it be considered as established that no other method can satisfy the so-called conditions of possible experience. The proof of the former proposition has, however, been carried sufficiently far to leave no doubt of its truth, and the latter one has in its favour the negative evidence furnished by the failure of solutions attempted by competent analysts upon other grounds.

2. On New Species of Marine Diatomaceæ from the Firth of Clyde and Loch Fine. By Professor Gregory. Illustrated by numerous drawings, and by enlarged figures, all drawn by Dr Greville.

In two papers, read before this Society, and subsequently published in the Microscopical Journal, I described and figured a large number of new species of Diatoms, chiefly marine, which I had found in the Glenshira sand.

This sand was deposited by the Dhu Loch of Glenshira, at a period geologically recent, when that lake occupied a higher level than it now does, and extended about two miles farther up the valley. That the Dhu Loch at that period, as well as now, communicated with Loch Fine, so that at high tide the salt water flowed into the lakes, while at low water the current, as in a tidal estuary, flowed outwards, is proved by the fact, that the sand then deposited contains more marine than fresh-water species. In the deposit now forming in the Dhu Loch marine forms are also abundant.

But while it was obvious that all the marine forms of the Glenshira sand had come from Loch Fine, itself a branch of the Firth of Clyde, it was remarkable that the new forms I had described should not have been found in the Clyde by those who had examined its deposits. There was indeed one form which I had figured, namely *Navicula Hennedyi*, which Mr Hennedy had shortly before observed in the Clyde, but which had not yet been described. Many known forms were also common to the Glenshira sand and to the Clyde.

Being firmly convinced that the new forms had also, so far at least as they were of marine origin, come from the Clyde, I resolved to explore such Clyde deposits as I could obtain; and having procured several, the result has entirely confirmed my anticipations, and has, besides, brought to light a large number of additional undescribed forms.

The materials I have examined are 11 in number, of which, 3 were from Lamlash Bay, one from *Corallina officinalis*, taken from pools on the shore of Arran at Corriegills, and 7 from Loch Fine, four of these having been dredged by the Duke of Argyll and myself off Inveraray, and 3 having been dredged off Strachur by the Rev. Dr Barclay.

For one of the dredgings from Lamlash Bay I am indebted to Professor Allman; the others from that quarter were sent to me by the Rev. C. P. Miles, M.D.

All were more or less interesting. The richest was that of Professor Allman, which was simply the dirt washed from some nests of Lima hians, dredged in 4 fathoms. One of Dr Miles's was also from these nests in 7 fathoms, and was not so rich, though still full of new forms. Those from Loch Fine were from depths varying from 14 to 60 fathoms.

No two were alike, except that the two just mentioned, from the nests of Lima, in nearly the same locality, were more like each other than any of the others. This variety is very curious, when we reflect that over a large extent of the bottom of the Atlantic the recent soundings have exhibited an astonishing similarity, being, however, entirely different from our estuarial deposits of the Clyde, and very poor in Diatoms.

The variety just alluded to shows that the deposits of as many localities as possible, even in the same estuary, should be examined, and that we cannot beforehand know what they are likely to yield. It is not at all likely that 11 dredgings, from three localities, and all different, should have exhausted the undescribed forms of the Firth of Clyde.

I. These materials yielded a very large number of known species, among which were a good many which have hitherto been extremely rare. Such are, for example,

Navicula Hennedyi, Sm.
„ granulata, Bréb.
„ Lyra, Ehr.
Pleurosigma transversale, Sm.
„ obscurum, Sm.
„ delicatulum, Sm.
„ rigidum, Sm.
Stauroneis pulchella, β Sm.
Coscinodiscus concinnus, Sm.
Eupodiscus crassus, Sm.
„ Ralfsii, Sm.
Eupodiscus sculptus, Sm.
Campylodiscus Horologium, Sm.
Podosira Montagnei, Sm.
„ maculata, Sm.
Orthosira marina, Sm.
Surirella lata, Sm.
„ fastuosa, Sm.
Biddulphia Baileyi, Sm.
„ turgida, Sm.
Grammatophora macilenta, Sm.
Syndendrium Diadema, Ehr.

Perhaps the most interesting among these forms, is *Campylodiscus Horologium*, which very few observers had seen, as it had only occurred very sparingly in a dredging made on the Coast of Skye by Mr G. Barlee, and examined by Professor Williamson. I have now a tolerably abundant supply of it, chiefly from Loch Fine, but it occurs also in Lamlash Bay. *Eupodiscus sculptus* occurs twice or three times the size of Professor Smith's figure, and *Eupodiscus Ralfsii* occurs, in the Corallina material, of remarkable size and beauty, and in great abundance. Discs of this fine species, from 0·005″ to 0·007″ in diameter, are frequent, and some even reach the diameter of 0·008.″ *Orthosira marina* (*olim Melosira sulcata*) is very abundant in Lamlash Bay. *Syndendrium Diadema*, figured by Mr Brightwell in his paper on Chætoceros, is not rare in Lamlash Bay. It has not, I believe, been yet described as a British species.

II. As I anticipated, these materials have yielded almost every one of the new forms of the Glenshira sand.

I have recognised the following :—

Navicula maxima, α and β
„ angulosa, α and β
„ latissima.
„ humerosa.
„ clavata.
„ incurvata.
„ splendida.
„ didyma γ, costata.
„ didyma δ
„ formosa.
„ rhombica.
Pinnularia Pandura, Bréb.
„ inflexa.
„ longa.
„ fortis.
Cocconeis distans.
Cocconeis costata.
Amphiprora recta.
„ lepidoptera.
„ minor.
Amphora Arcus
„ Grevilliana.
„ obtusa.
„ crassa.
„ plicata.
„ elegans.
„ lineata.
„ rectangularis.
Tryblionella constricta.
„ apiculata.
Campylodiscus simulans.
Synedra undulata.

It is impossible to doubt, that the few remaining Glenshira marine forms will yet be found in the Clyde, perhaps even in these dredgings ; if not, in others.

III. Besides the above forms, all of which I had figured, there had occurred in the Glenshira sand 7 or 8 forms, the study of which I had postponed, either from want of good specimens, or from their extreme scarcity in that deposit. By the help of the new materials, I have been enabled to clear up, and to establish as distinct species, every one of these forms, most of which are very curious and interesting.

IV. Lastly, I have detected, in these materials, a very large number of new species. These I have arranged in the following groups.

GROUP I.

Naviculoid Forms.

1. Navicula minor, n. sp.
2. „ Cluthensis, n. sp.
3. „ inconspicua, n. sp.
4. „ brevis, n. sp.
5. Navicula Claviculus, n. sp.
6. „ Musca, n. sp.
7. „ rectangulata, n. sp.
8. „ nebulosa, n. sp.

9. Navicula Barclayana, n. sp.
10. „ spectabilis, n. sp.
11. „ prætexta, Ehr.
12. „ Bombus, Ehr.
13. „ Lyra, Ehr.
14. „ Lyra, var. β, abrupta.
15. „ Smithii, var. β, fusca.
16. Navicula Smithii, var. γ, nitescens.
17. „ Smithii, var. δ suborbicularis.
18. „ maxima Greg.
19. Pinnularia subtilsi, n. sp.
20. „ rostellata, n. sp.
21. „ Allmaniana, n. sp.
22. „ Pandura, Breb, var.

I have given a figure of the typical *N. Lyra*, Ehr., not yet figured as a British form. The figure referred to by Professor Smith in his second vol. (vol. i. fig. $152\overset{*}{a}$) is that of the variety β.

I exhibit drawings of all the most remarkable of these forms, and an enlarged figure of *N. prætexta*, Ehr., which is not only remarkable and beautiful in itself, but interesting from the circumstances in which it occurs.

Ehrenberg found it only fossil, in the Clay Marl of Ægina, where it seems to be very scarce, as he has figured an imperfect specimen. I have found it recently, both in Lamlash Bay and Loch Fine, and though not abundant, yet sufficiently frequent to have enabled me to distribute a good many specimens. No doubt, if we persevere in examining the estuarial deposits, we shall some day find it in greater abundance in the vicinity of its proper habitat.

Here, then, is a form which, till now, has been regarded as fossil only, which is found to be still existing in the Clyde. The Clay Marl of Ægina is stated by Ehrenberg to belong either to the chalk formation, or to the oldest tertiary or eocene beds. I have selected this form, because the bed in which it occurs fossil is the oldest in which Ehrenberg has found any Diatoms. He has, indeed, found microscopic organisms in the chalk, and even in older rocks, among which he mentions the Mountain Limestone and the Silurian Green Sand. But the forms in the two latter rocks are not numerous, and, as well as those which abound in the chalk, belong to the Foraminifera or to the Polycystineæ, not to the Diatomaceæ.

We find, then, that this very remarkable form, which occurs fossil in the Clay Marl of Ægina, exists in the Clyde at the present day; and there is no difference whatever between the fossil and the recent specimens.

But this is not all. In the same Clay Marl Ehrenberg has figured many more species of Diatoms, and of these, upwards of three-fourths are absolutely identical with forms which abound in the Clyde. Such are *Navicula Bombus*, *N. incurvata*, *Pinnularia Pandura*, *Orthosira marina*, *Amphitetras antediluviana*, *Tricer-*

atium Favus, *Actinocyclus undulatus*, *Coscinodiscus radiatus*, several Grammatophoræ, and various others.

In short, I have no hesitation in saying, that I believe all the forms in the Ægina Clay Marl, which is the oldest Diatomaceous deposit yet described, will be found living on our coasts.

It may also be observed, that of all the forms figured by Ehrenberg from more recent strata, whether miocene, like the bed on which the town of Richmond, Virginia, is built, and several kinds of Bergmehl, or pleiocene, like other Bergmehls and polishing slates, &c., or still more recent, the great majority are perfectly identical with existing Diatoms.

Indeed, although many forms are stated in Ehrenberg's earliest writings to be fossil only, and have been supposed to be extinct, the progress of observation is continually adding to the number of species which are found also in the recent state. Thus, for example, the whole group of dentate Eunotiæ, which abound in the Lapland and Finland Bergmehls, were long thought to be only fossil. But they have been nearly all found living in America, and I have myself seen several of them recent in this country. *Eunotia triodon*, long supposed to be extinct, occurred scattered in many of the Scottish fresh-water gatherings I described in this place three years ago, and I found it this last summer the predominant form in a gathering brought from Arran by Dr Balfour.

Taking these facts into consideration, I am led to believe that we have no evidence that any species of Diatom has become extinct, as so many species, and even genera and tribes, of more highly organized beings have done. I observe that Mr Brightwell expresses a similar opinion in his valuable paper on Chaetoceros. (See Micr. Jour. IV. 105.) His anticipation that Syndendrium would be found recent has been fulfilled. (See List, p. 450.) Our knowledge of the existing species is yet very imperfect, as is obvious from the facts adduced in this paper, in which so many undescribed forms are shown to exist in a few localities of one estuary. And among these are such forms as the present one, *N. prætexta*, which has hitherto been supposed only to exist as a fossil.

It is well known, that in certain animal tribes, Molluscs, for example, many species are common to the present and to earlier geological periods. I need only allude to the circumstances that the eocene, miocene, and pleiocene strata are named from their proportions of existing to extinct molluscs, and that the Terebratula of the Silurian epoch is found to exist at the present day.

But in the case of Diatoms, there is reason to think that the whole of the species which occur fossil will, ere long, be detected in the recent state, just as has occurred in the case before us, *N. prætexta*, which, it must be remembered, occurs in the oldest Diatomaceous deposit yet described, and along with forms, nearly all of which I have actually found in the Clyde.

It is at all events certain, that a very large proportion of the Diatoms found in the fossil state also occur in the living state, and that every day adds to their number.

There is at present no good evidence of the existence of Diatoms earlier than the Chalk, if so early. But we must not forget that the shells of Diatoms appear to be altered by long contact with carbonate of lime, so that they may have existed at one time in the Chalk. We find them, however, in spite of the action of calcareous matter, in the Chalk Marls of Meudon and of Caltanisetta, which are rather more recent than the Chalk, and probably about the age of the Clay Marl of Ægina. If, as I believe, no Diatoms have become extinct, this may, perhaps, depend on their minute size and extreme simplicity of structure, which probably render them more indifferent to climatic changes than more highly organized and larger beings.

We have evidence, to a certain extent, that this is the case; for by Ehrenberg's figures it appears, that in gatherings of recent Diatoms, from all parts of the world, in every possible variety of climate, the majority of the species are identical with our own.

Diatoms, therefore, are not materially affected by existing differences of climate, and have probably been as little affected by the geological changes which have occurred, at all events since the period of the eocene deposits.

To return to the new forms.

Group II.

Cocconeides.

The number of new forms in this group is not large, but they are all interesting. They are as follows:—

23. Cocconeis distans, W.G.
24. „ dirupla, n. sp.
25. „ ornata, n. sp.
26. „ nitida, n. sp.
27. Cocconeis pseudomarginata, n. sp.
28. „ major, n. sp.
29. „ splendida, n. sp.

The first I have figured a second time, because I have detected in it a character which effectually distinguishes it from *C. Scutellum*. The second is one of those Glenshira forms, which I have been enabled by the new materials to study and distinguish. I exhibit drawings of them, and enlarged ones of *C. nitida*, *C. splendida*, and *C. pseudomarginata*, which are very striking forms. *C. major* is equally remarkable.

GROUP III.

Filamentous Forms.

Of these, there are a good many, and most of them are curious.

30. Diadesmis ? Williamsoni=Himantidium Williamsoni, Sm.
31. Denticula marina, n. sp.
32. „ distans, n. sp.
33. „ minor, n. sp.
34. Denticula ? nana, n. sp.
35. „ ? fulva, n. sp.
36. „ ? staurophora, n. sp.
37. Denticula ? lævis, n. sp.
38. „ ? capitata, n. sp.
39. „ ? interrupta, n. sp.
40. „ ? ornata, n. sp.
41. Meridion ? marinum, n. sp.
42. Pyxidicula cruciata, Ehr.
43. Orthosira angulata, n. sp.

The first species was described by Professor Smith, but doubtfully, as a Himantidium, the F.V. only being then known. The S.V., which abounds in some of the dredgings, proves that it is not a Himantidium ; but it is not so easy to say to what genus it belongs. Diadesmis is not admitted by Professor Smith, but comes nearer to it than any of his genera. It has also some analogy with Achnanthes, as well as with Odontidium and Denticula. I give the genus, therefore, with a mark of doubt. The four next agree pretty well with Denticula ; but the six which follow them are all very doubtful as to genus, although, perhaps, nearer to Denticula than to any other genus admitted by Smith. The next form has strong analogies with Meridion, and even with Gomphonema. I do not venture here to decide on the genera of these forms, but content myself with indicating the existence of the species. *Pyxidicula cruciata* is a form, long described by Ehrenberg as fossil; indeed it occurs in the Ægina Clay Marl, already mentioned; and in the miocene deposit of Richmond, Virginia. I do not know that it has ever been found recent, till now. *Orthosira angulata* is very abundant in Lamlash Bay, and its disc has probably been described as *Coscinodiscus minor*, by Kützing and others. But it is a true Orthosira.

GROUP IV.

Discs and Campylodisci.

These, though not very numerous, are very interesting—

44. Coscinodiscus centralis, Ehr.
45. „ nitidus, n. sp.
46. „ umbonatus, n. sp.
47. „ punctulatus, n. sp.
48. „ concavus, Ehr.
49. Melosira ? n. sp.
50. Eupodiscus subtilis, Ralfs.
51. Campylodiscus Ralfsii, Sm.
52. „ centralis, n. sp.
53. „ angularis, n. sp.
54. „ eximius, n. sp.
55. „ limbatus, Bréb.

It will be seen, that two of these forms have been described by Ehrenberg, who figures them in the fossil deposits above alluded to, and one by De Brébison, who found it near Cherbourg. The form to which Mr Ralfs's name is attached was supposed by him to be *Coscinodiscus concinnus*, Sm., but proves to be a very remarkable Eupodiscus. I had not seen it when Mr Ralfs first observed it; but since then, Dr Greville observed it on one of my slides, and I have myself repeatedly noticed it since. I give a figure of it, as it has not yet been figured. *Campylodiscus Ralfsii* I figure, because it occurs in these dredgings, twice or thrice the size of Professor Smith's figure, from which, moreover, it differs in several points; but I believe it to be the same species. The remaining forms are new and remarkable. That which I suppose to be a Melosira is doubtful as to genus.

GROUP V.

Amphiproræ.

There are not many new species of this genus, but all of them are remarkable.

56. Amphiprora lepidoptera, n. sp.
57. „ obtusa, n. sp.
58. „ pusilla, n. sp.
59. „ elegans, Sm.
60. Amphiprora plicata, n. sp.
61. „ maxima, n. sp.
62. „ ? complexa, n. sp.

The first was described, and the F.V. figured in my last paper on the Glenshira sand; while the S.V. had been figured in my first paper as *Amphiprora vitrea β*. But as both figures were imperfect, and the form not quite understood, I have now figured it again. The remaining species, except No. 59, are all new and distinct, and *Amphiprora maxima* is a splendid form.

The last is a very remarkable form, which I doubtfully refer to the same genus. Segments of it had occurred in the Glenshira sand, but it was only in the Corallina gathering that I found the entire form. I exhibit enlarged figures of *Amphiprora maxima*, and of *Amphiprora ? complexa*, both the segments and the entire form.

GROUP VI.

Amphoræ.

This group is by far the largest, containing upwards of thirty new species, in addition to the new Amphoræ of the Glenshira sand, which, as already stated, also occur in these materials. I have found it necessary to divide them into two series, the simple and the complex.

A.—Simple Amphoræ.

63. Amphora turgida, n. sp.
64. „ augusta, n. sp.
65. „ nana, n. sp.
66. „ macilenta, n. sp.
67. „ lineata, Greg.
68. „ ventricosa, n. sp.
69. „ binodis, n. sp.
70. „ monilifera, n. sp.
71. „ Ergadensis, n. sp.
72. Amphora oblonga, n. sp.
73. „ spectabilis, n. sp.
74. „ robusta, n. sp.
75. „ truncata, n. sp.
76. „ lævis, n. sp.
77. „ lævissima, n. sp.
78. „ dubia, n. sp.
79. „ Proteus, n. sp.
80. „ pellucida, n. sp.

B.—Complex Amphoræ.

81. Amphora Arcus, Greg.
82. „ crassa, Greg.
83. „ Grevilliana, Greg.
84. „ complexa, n. sp.
85. „ fasciata, n. sp.
86. „ sulcata, Bréb.
87. „ excisa, n. sp.
88. „ nobilis, n. sp.
89. „ Milesiana, n. sp.
90. Amphora lyrata, n. sp.
91. „ proboscidea, n. sp.
92. „ cymbifera, n. sp.
93. „ quadrata, n. sp.
94. „ elongata, n. sp.
95. „ acuta, n. sp.
96. „ pusilla, n. sp.
97. „ bacillarus, n. sp.
98. „ granulata, n. sp.

I exhibit enlarged figures of one or two from each division.

The first three species in the list of complex Amphoræ have been already figured from the Glenshira sand, but imperfectly, and in one case erroneously; I therefore figure them again, as they are now better understood. This complex group, of which a short time since only one was known, namely,—*A. costata*, Sm., has now become a very large one, and the remarkable structure of the forms included in it, which had hardly been attended to in *A. costata*, is found to be of very frequent occurrence. It is probable that these complex forms will require the establishment of a new genus; but in the meantime I regard them as forming a well-marked sub-genus.

There are still several species of Amphoræ to be added to the long list already given; but they have not yet been fully studied, for want of time.

GROUP VII.

Miscellaneous.

A few forms are here added, which do not enter into the groups already named, or were observed too late.

99. Navicula? Libellus, n. sp.
100. Nitzschia? panduriformis, n. sp.
101. Nitzschia distans, n. sp.
102. „ hyalnia, n. sp.
103. Pleurosigma reversum, n. sp.
104. Sceptroneis Caduceus, Ehr.
105. Synedra undulata, Greg.
=Toxarium undulatum, Bail.
106. Synedra Hennedyana, n. sp.

The first of these forms resembles *N. rhombica*, figured as occurring in the Glenshira Sand. Both forms are doubtful as to genus, and may possibly prove to be Schizonemata. The second is allied to Tryblionella as well as to Nitzschia. The fifth, *Pleurosigma reversum*, is a very singular form, the genus of which is not quite certain. The sixth is of a genus new to Britain, and is one of the forms hitherto believed to be fossil only. The next, *Synedra undulata*, has not yet been figured entire as a British form. The form which follows closely resembles it except that it has no undulations in the margin.

In an appendix, I have added a full description and a figure of a very fine new species, detected by Professor Arnott in a gathering from Teignmouth, and subsequently found, by the same observer, in one from the Clyde, which justifies its introduction here. The figure and description are by Dr Greville.

The new form, *Creswellia Turris*, Arnott, belongs to an entirely new genus, which is allied to Pyxidicula, but differs from that genus as described by Ehrenberg, in forming filaments or chains. It will be remembered, that I have also detected a Pyxidicula in the Clyde, so that both these allied genera occur in that estuary.

3. Short Verbal Notice of a simple and direct method of Computing the Logarithm of a Number. By EDWARD SANG, Esq.

Mr Sang briefly explained an application of the method of continued fractions to the resolution of the exponential equation, and illustrated it by exhibiting the computation of the logarithm of the prime number 27073 directly.

The following Donations to the Library were announced :—

Publications of the Ælfric Society, viz.—

1. The Homilies of Ælfric, with an English Translation. By Benjamin Thorpe, F.S.A. 2 vols. 8vo.—London, 1843–1846.
2. The Poetry of the Codex Vercellensis, with an English translation. By J. M. Kemble, M.A. 8vo.—1844–1856.

3. Anglo-Saxon Dialogues of Salomon and Saturn. By John M. Kemble, M.A. 8vo.—1845–1848.

From William Ivory, Esq., W.S.

Catalogue of the Law Books in the Library of the Society of Writers to Her Majesty's Signet in Scotland. By William Ivory, W.S. 8vo.—Edinburgh, 1856. *From the Author.*

Jahresbericht über die Fortschritte der reinen, pharmaceutischen und technischen Chemie, Physik, Mineralogie und Geologie. Herausgegeben von Justus Liebig und Hermann Kopp, 1855, Zweites Heft. 8vo. *From the Editors.*

Memorias de la Real Academia de Ciencias de Madrid, Tom. III. and IV. 4to.—Madrid, 1856. *From the Academy.*

Programa para la adjudicacion de premios en el ano 1857. *From the same.*

Anuncio del Eclipse anular y Central que tendra lugar el 15 de Marzo de 1858. Por Don Antonio Aguilar. 8vo.—*From the Author.*

Assurance Magazine, and Journal of the Institute of Actuaries, No. XXVI., January 1857.—*From the Institute.*

Percement de l'Isthme de Suez. Rapport et Projet de la Commission Internationale. 8vo.—Paris, 1856. *From the International Suez Canal Company.*

Flora Batava. 180 Aflevering. 4to.—*From the King of Holland.*

Monday, 2d February 1857.

THE RIGHT REV. BISHOP TERROT, V.P., in the Chair.

The following Communications were read:—

1. On the Urinary Secretion of Fishes, with some remarks on this secretion in other classes of animals. By John Davy, M.D., F.R.S., London and Edinburgh.

The urinary secretion of fishes, the author believes, has hitherto received so little attention owing to certain difficulties attending its investigation. He brings forward the few and imperfect observations he has made, with the hope of inducing others more favourably situated to prosecute the inquiry.

The fishes he has examined in quest of their urinary secretion have been fifteen; of those with a urinary bladder he found a fluid only in three, the Perch, Ling, and Ray; and in those without this organ, only in two, in their ureters—those of the Pike and Turbot.

His experiments to ascertain the composition of the secretion were attended mostly with negative results. In one instance, that of the Pike, he detected lithic acid; in some others there were indications of the presence of urea in the fluid urine.

The conclusions he ventures to draw are, that the secretion is small in quantity, mostly liquid, and that urea or some other analogous nitrogenous compound is its principal ingredient.

For the sake of comparison and further inferences, he notices the secretion in other classes of animals, laying emphasis on the proposition, that the quality of the secretion in each is more regulated by the structure of the urinary organs than by any other circumstance, and that the quality of the food, whether animal or vegetable, neither exercises an influence on the quantity of the nitrogenous excretion nor on its kind. He points out how, irrespective of the nature of the food, urea is the chief ingredient of the urine of the Mammalia, and of other animals provided with a urinary bladder; how the alkaline lithates and uric acid take the place of urea in those destitute of this organ, such as birds, serpents, lizards, insects, mollusca, and some lower in the scale of organization, as the myriopoda; and further, how in the instance of scorpions and spiders another compound is substituted for the soluble urea, viz., guanine, which, like the lithates, is excreted in the state of a soft solid.

He concludes with adverting to the harmonious relation of functions in the animal economy, and the close alliance in action between the lungs and the kidneys; adding, that as their activity seems generally to be in accordance, and as in fishes the temperature is low, and little carbonic acid evolved, it may be presumed, as his experiments seem to show, that their urinary secretion is proportionally inconsiderable. And hence, as their digestive powers are great, it may further be inferred that most of their food is assimilated, and is used in administering to their growth. This view he thinks is in agreement with, and helps to explain some of their peculiarities, such as we witness in the salmon, the history of which, of late years, has been so carefully and successfully studied.

2. On the Reproductive Economy of Moths and Bees; being an Account of the Results of Von Siebold's Recent Researches in Parthenogenesis. By Professor Goodsir.

In a recent work, entitled "Wahre Parthenogenesis bei Schmetterlingen und Bienen"—Real Parthenogenesis in Butterflies and Bees, Von Siebold, after reviewing the present condition of the subject, distinguishes the so-called alternate generation from parthenogenesis, and limits Professor Owen's term to reproduction by unimpregnated females.

He has ascertained that this real parthenogenetic mode of reproduction occurs in certain moths—*Solenobia lichenella* and *triquetrella*, the larvæ of which are sac-bearers; and in *Psyche helix*, the larva of which constructs one of those remarkable spiral sacs which have been mistaken for the shells of molluscs.

It would appear that the apterous females of these moths reproduce for many generations without the access of the male, and that the progeny of these virgin females is female.

Von Siebold's observations on the bee larvæ resulted in the verification of the hypothesis recently propounded by Dzierzon, a clergyman and distinguished bee-master in Southern Germany, that *all the eggs which come to maturity in both ovaries of the queen bee are of one kind, which, if deposited without having come in contact with spermatic fluid, are developed into drone or male bees; but if impregnated by spermatic contact, are developed into female, —that is, into working or queen bees, according to their subsequent treatment*. The verification of this remarkable and important physiological doctrine has apparently been effected by, 1, The admitted fact, that working bees occasionally deposit drone eggs; 2, By the determination of the voluntary muscular structure of the spermatheca; 3, By the detection of spermatozoa in the interior of eggs recently deposited in working comb-cells, and the non-detection of spermatozoa in eggs deposited in drone comb-cells; and, 4, By a careful analysis of the entire reproductive economy of the bee.

Von Siebold, therefore, considers himself entitled to conclude that the females of certain lepidoptera, and the males of the bee, are developed from unimpregnated ova.

3. On the Principles of the Stereoscope; and on a new mode of exhibiting Stereoscopic Pictures. By Dr W. Macdonald.

The following Gentleman was admitted an Ordinary Fellow:—

Dr John Ivor Murray, F. R. Coll. Surg. Edin.

The following Donations to the Library were announced:—

Journal of the Asiatic Society of Bengal, No. V. 8vo.—*From the Society.*

Catalogue of Stars near the Ecliptic, observed at Markree during the years 1854-56, and whose places are supposed to be hitherto unpublished. 8vo. Vol. IV. (containing 14,951 stars.)—*From H. M. Government.*

Supplément aux Comptes Rendus Hebdomadaires de Seances de l'Académie des Sciences. Tom. I. 4to.—*From the Academy.*

Mémoires de l'Académie des Sciences de l'Institut Imperial de France. Tom. XXVII. 1re partie. 4to.—*From the Academy.*

Mémoires présentés par divers Savants à l'Académie des Sciences de l'Institut Imperial de France, et imprimes par son ordre. Sciences Mathematiques et Physiques. Tom. XIV. 4to.—*From the Academy.*

Proceedings of the Zoological Society. 8vo. Nos. 310–313. —*From the Society.*

Quarterly Journal of the Chemical Society, No. 36, January 1857. 8vo.—*From the Society.*

Nouveaux Mémoires de la Société Helvetique des Sciences Naturelles. Band XIV. 4to.—*From the Society.*

Mittheilungen der naturforschenden Gesellschaft in Bern, für 1855, 1856. 8vo.—*From the Society.*

Actes de la Société Helvetique des Sciences Naturelles, 1855. 8vo.—*From the Society.*

Verhandlungen der allgemeinen schweizerischen Gesellschaft für die gesammten Naturwissenschaften bei ihrer versammlung in St Gallen am 24, 25, und 26 Juli 1854. 8vo.—*From the Society.*

Comptes Rendus hebdomadaires des Séances de l'Académie des Sciences, May 1856—December 1856.—*From the Academy.*

Monday, 16*th February* 1857.

Dr CHRISTISON, Vice-President, in the Chair.

The following Communications were read:—

1. On the Crania of the Kaffirs and Hottentots, and the Physical and Moral Characteristics of these Races. By Dr Black, F.G.S.

After exhibiting Crania of these races recently received from South Africa, along with two British crania of normal size and form, and stating their respective measurements, facial angles, and principal diameters, the author reviewed the observations of Natural Historians and Anatomists on the skulls and physical characteristics of the races above mentioned. Camper's facial angle was compared with that of Blumenbach, showing the differences, and how far the adult Negro and the orang-outang receded from the higher cranial types; and also how, according to Mr Owen, the facial angle depends upon whether it is taken in the young or adult state. The little difference which exists between the capacities of the skulls of most of the human races was alluded to, and also Mr Owen's opinion of the superiority of the basal examination of the skull over that of the facial angle or vertical aspect. A peculiarity of several Negro skulls was also mentioned, viz., the juncture of the temporal and frontal bones, and consequently the separation of the sphenoid from the parietal. It is observed, that the people of the high countries of Africa are greatly superior to those of the low plains; the former receding further from the physiognomy and colour of the Negro. Colour of skin greatly varies amongst the South African natives, from black to a light brown, even amongst the same nation. The superiority of strength of intellect possessed by the Kaffir over the Hottentot is sufficiently marked; and, on the other hand, is counterbalanced by the greater pliability of disposition in the latter, the one being remarkable for reasoning abilities, and the other for aptitude for music, &c.

M. Virey's description of the distinguishing traits of the Kaffirs and Hottentots was noticed, and the mental superiority of the former over the latter. The Kaffirs are held by him, Prichard, and Knox, to be of one ethnic origin with the Negroes, only they are improved, and may be called the Negroes of the mountains, a change being caused by inhabiting an extra-tropical climate.

Dr Morton and the Editors of the "Types of Mankind" were noticed as observing the affinities between the Kaffirs and the Foolahs and Felatahs,—an affinity upon which others have thrown a doubt; while the Editors agree with other observers that the Kaffirs resemble the true Negro much more than the Hottentot, and that the latter is supposed to belong to the same race as the Bosjesman. The great question of the primitive origin of mankind was next adverted to, and it was stated that, in opposition to Dr Prichard, the Editors of the "Types" contend for distinct zoological creations, grounding their opinions partly on Egyptian monuments.

Richard Lander has made personal observations on the Kaffirs, and has shown the resemblance between them and the Felatahs; and Lichtenstein concludes, that the same race of people and the same family of languages were spread into the interior, and also toward the north, far beyond the limits of the Kaffirs proper. Dr Kraff and Peters were quoted, as showing that all the eastern nations, from the equator to the Cape colony, belong to one great family.

The opinion that the Kaffirs ought to be classed with the Europeans or Arabs was shown to be unsound, and also that they considerably recede in features and shape of the skull from the *prognathous* races. There is one trait that directs us to a foreign source, namely, the rite of circumcision, which they universally practise. It is known that the ancient Africans and Egyptians practised the same rite. After noticing the belief of the Kaffirs in a Supreme Being, though they have no idea of future rewards and punishments, the author noticed the Bechuana Kaffirs as superior to the Amakosae, and as having made some advance in arts and civilization, and the Kosah Kaffirs of the pure breed as approaching in colour and physiognomy to the ordinary Negro, but some of them are of a lighter hue, and redder. Colonel Smith's observations on the Kaffir race are favourable to their intelligence and bravery, and some of them display the peculiar accumulation of fat over the *glutei* muscles which is more common among the Hottentots and Bushmen. He also mentions that the northern tribes try to mitigate the small-pox by inoculating between the eyes. Further testimony seems to establish a near and kindred relation between the languages of South Africa, the idioms of which constitute a particular family of languages, and a singular development of human speech. In one respect these languages are shown to agree with the Coptic—in the law by which they both *prefix* all modifying particles, while other languages *suffix* them.

The author then entered briefly into a description of the Fingoes and Bosjesmen, as given by travellers and natural historians, showing that the latter exhibit the features of the lowest humanity, and are almost allied to the ourang and troglodytes; though Dr Smith's more accurate observations have convinced Dr Prichard that the Bosjesmen are of the same race as the Hottentots, and originally spoke the same language. The oblique foramen at the lower end of the humerus was also noticed as occurring in some of them, as it does in the humeri of the simiæ, dogs, and wild boar. Observations were made on the physical form and features of the Hottentots, including the form of their noses, cheek-bones, chin, eyes, and teeth, their hair and colour, their nipples and mammæ at different ages, and also the *steatopygea* of the females. While some consider them a tribe of Mongolians, and allied to the Kalmucks, others are inclined to think them quite as primitive in their origin as these Asiatics.

The author concludes: "Notwithstanding the difficulties which ethnographists must meet with in assigning a probable origin and descent to the Kaffir, and especially to the Hottentot tribes, sufficient to induce them to resort to the theory of distinct centres of human, as well as of animal and vegetable creation; and, notwithstanding all that has been lately advanced on the subject of the unity of the human species, we may safely say with the eminent Natural Historian, that no differences, such as those observed among mammiferous animals, are to be found among human families, and that whatever varieties exist in these respects are the effects of external agencies, and the tendency to variation which such agencies call into activity."

2. On a *Roche Moutonneé* on the summit of the range of hills separating Loch Fyne and Loch Awe. In a letter from the Duke of Argyll to Professor Forbes.

Polished and rounded surfaces of rock are, under their more ordinary conditions, of very frequent occurrence in Argyllshire. By "their more ordinary conditions," I mean principally two— viz., Where they occur on the existing coast-line, either at, or not far above the present level of the sea; secondly, Where they occur in valleys, or the lower flanks of the hills,—whether under the boulder clay, or on surfaces naturally exposed.

From the occurrence on Loch Fyne of some beds of a chloritic schist, which is both very soft and very tough, receiving easily mechanical impressions, and resisting as easily the chemical agencies which tend to wear them off, there are on its shores some very fine examples of rocks rounded and deeply grooved. So far as my observation goes, however, they all indicate not only a configuration of the surface identical with that which now exists, but also a comparatively small change in the level of the sea. The prominent points upon the shore are those which are principally marked—the direction indicated is from the head of the loch towards its mouth; that is to say, the direction which ice breaking up from the high mountains at the head of the lake would naturally take in its passage to the open sea. There is nothing to imply a change of level beyond some fifteen or twenty feet.

In the glens leading into Loch Fyne, in the neighbourhood of Inverary, the stones and boulders in the clay are abundantly marked by abrading action. But I have not observed upon the walls of those glens any appearances which would indicate direct glacier action. When smoothed surfaces of rock appear at all, they seem rather connected with the general phenomena of the Boulder Clay. There is one observation, however, which applies generally, if not universally, to smoothed surfaces of rock in all these situations— viz., that the direction of the striæ is that of the glen or valley in which the rock is situated, that is to say, they are in the direction which currents of water, whether bearing ice or stony matter, or both, would naturally take, if guided in any degree by the existing configuration of the surface.

What appears to me to be the peculiarity of the "Roche Moutonnée" I am now about to describe, is its position, forcing us to seek for its explanation in causes with which the physical geography of the country can have had comparatively little to do.

In a communication to the Geological Society of London which I made about three years ago, with reference to another subject, I gave a general description of the structure of the ridges which separate the valley of Loch Fyne from that of Loch Awe. This line of hills, taken from the point where it is traversed by Glenaray, runs in a south-westerly direction about twenty miles, till it falls into the transverse valley behind Lochgilphead, along which the Crinan Canal is carried from Loch Fyne to the Western Sea. It is of moderate elevation as compared with the great group of mountains which lies to the N.E. But these mountains, closing round the upper end of Loch Awe, terminate in Ben Cruachan, opposite to the point which I have taken as the commencement of the ridge referred to; and beyond that point the remainder of the northern shores of Loch Awe, and the whole district of country extending from it towards Oban, is of much lower elevation, so that, standing on the higher points of the range of hills to which I refer, there is an uninterrupted view to the channels which wind among the larger Hebrides to the mountains of Jura, Colonsay, Mull, and up the Linnhe Loch to the entrance of the great valley of the Caledonian Canal.

It was on going to one of these points—the highest on the ridge for some miles, and probably about 1800 feet above the level of the sea—during last autumn, that I was surprised to observe close to the summit so remarkable an example of a well-rounded surface of rock as to attract my attention from a considerable distance. It is the more remarkable, from the contrast it presents with the generally sharp and broken edges of the strata, which are there composed of hard quartzose beds, at a high inclination, and with steep escarpments. The direction from which the abrading force has acted is about N.N.E. It has passed over a lower shoulder in its way—lower by about 100 feet; but the effect is strongest upon the rocks of the main peak itself, and especially upon one or two prominent faces within twenty or thirty feet of the summit. Above this it may be observed sloping off, as it were, with diminished force, over successive ledges towards the top, until it passes close behind the very highest point, leaving that point itself apparently untouched.

I need hardly say, that in this case glacier action is impossible. Even if this hill had itself been the seat of the glacier, it could only have been snow so near the summit.

There is one explanation which immediately suggests itself to the mind, and, however difficult it may be to realize the conditions which it involves, it is the only one which it seems to me to be possible to suggest. It is that this peak, when subject to that grinding force, was a rocky islet just appearing above the surface of a glacial sea, and that floating icebergs, drifting from the north-eastward, were constantly grounding upon its sides. I may observe, that this explanation would accord with the fact that the surfaces displaying most strongly the effects of the abrading action are considerably below the summit; because, as ice floats deep in the water—not more than one-twelfth of its bulk being above the line of flotation—the heaviest masses would always ground upon such a rock at a distance from the top proportioned to the steepness of its declivity, and none but the lighter pieces would ever be drifted over the higher points.

If this peak was ever, as I have supposed, a mere rock just appearing above the level of the sea, it becomes of course necessary to suppose that its altitude above the present level of the ocean is due to subsequent elevation. But as the geological structure of the range—the dip and inclination of its rocks—were clearly the same when it was subject to these forces of a glacial sea, it becomes necessary further to suppose that the required elevation has been a general one, affecting the whole country of which this range forms a part. But if this were so, and whether the elevation was slow and continuous, or more rapid and effected by (as it were) successive heaves, we should expect to find at corresponding points evidences of the same action at lower and lower stages. There is certainly one such evidence, viz., that on some of the lower ridges which fall in successive steps to the shores of Loch Fyne there are some very remarkable examples of large blocks of granite perched upon the very summits, in positions which it is impossible to suppose them to have attained by any other means than transportation upon ice. Such blocks, if they had fallen or rolled from above, must have crossed deep hollows, and again bounded up steep slopes with such a nice adjustment of impetus as to exhaust itself exactly at the very top, leaving them perched, where perhaps a single step in advance would have precipitated them on another journey into the glen next below. But this position so far from being a difficulty, is but the natural position in which we should expect to find them, if ice had been the transporting agent; because, what are now summits must have been then shoals, upon which ice would ground, and upon which, also, ice melting, they would drop their burdens.

I send with this very short notice, a copy of the paper to which I have referred,* upon the geological structure of the line of hills on which this Roche Moutonnée occurs. In it there is an eye sketch, tolerably correct, taken from Loch Fyne, of the range of hills between its waters and Loch Awe, and I have marked in red ink the point where the smoothed surface occurs. On the succeeding page of the same paper there is an ideal section of the same range of hills, and on this I have likewise marked both the position of the rock and of the massive blocks of stone alluded to in this letter.

Since I came to London, my attention has been drawn by Sir Charles Lyell to a paper, in which he has described a similar phenomena in the United States, and in which he accounts for it by the same explanation. In that case, however, the fact of a Roche Moutonnée on the summit of a range of hills is accompanied by other very peculiar phenomena of transported boulders, which corroborate in a manner not to be mistaken the conclusion thus arrived at. The continuity and uniformity of direction taken by the floating ice in that case, as evidenced by the lines of its deposit, is a peculiarity which must have been due to local causes. But if the facts of submergence and elevation to such an extent be established anywhere, there is no difficulty in applying the same explanation to other cases, which, however different in detail, involve the same essential facts.

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* Quarterly Journal of the Geological Society for November 1853.

I need not allude to the questions which crowd upon us in contemplating the explanation thus suggested. What is the connection between the glacial period indicated by the Roches Moutonnées found close to the present level of the sea, and those —in all respects so similar—found on the tops of mountains of such considerable elevation. Was it one long glacial epoch during which the required elevation was accomplished? or was it a recurrence of similar conditions after a gale of intermediate changes?

3. On M. J. Nicklès' claim to be the Discoverer of Fluorine in the Blood. By George Wilson, M.D., F.R.S.E., Regius Professor of Technology in the University of Edinburgh.

I am very reluctant to occupy the time of this meeting with a personal matter, but as I am necessitated to defend my priority in reference to certain researches which, in greater part, were first communicated to this Society, and first made public through its "Transactions," it seems the proper tribunal, at least in this country, to adjudicate on a question liable to dispute.

A communication was made to the French Academy, at its meeting on the 3d of November 1856, by M. J. Nicklès, entitled "Presence du Fluor dans le Sang." From the tenor of M. Nicklès' remarks, it would seem that he is not aware that the existence of fluorine in the blood was announced by me in 1846, and specially demonstrated in 1850; nor is he acquainted with the researches which others besides myself have made in this country and in America, into the distribution of fluorine throughout the different kingdoms of nature. In justice, accordingly, to all parties, I seek to recal the following facts, which may save M. Nicklès needless labour, and prevent future disputes. His announcement is as follows: It is reported in the *Comptes Rendus* for November 6, 1856, and in the *Journal de Pharmacie et de Chimie*, December 1856, p. 406, from which I take it:—

"*Présence du Fluor dans le Sang.* Par M. J. Nicklès. (Communiqué à l'Académie des Sciences, dans la séance du 3 Novembre 1856.)

"Par suite de considérations que j'aurai prochainement l'honneur de soumettre à l'Académie j'ai été conduit à vérifier cette assertion tant contestée, de la présence de fluor dans les os. Mes expériences ayant été affirmatives j'ai recherché le fluor dans le sang, seule voie par où il ait pu arriver jusqu'au tissu osseux. J'y en ai trouvé de notables proportions, non pas seulement dans le sang humain, mais encore dans celui de plusieurs mammifères (porc, mouton, bœuf, chien), et de plusieurs oiseaux (dindon, oie, canard, poulet).

" Des résultats si concordants me semblent donner au fluor une importance qu'il n'a pas eue jusqu'à ce jour en médecine ou en physiologie ; ils infirment évidemment cette opinion de Berzélius, suivant laquelle la présence du fluor dans les os est purement accidentelle, et qu'en tout cas elle n'est pas nécessaire.

" S'il fallait d'autres preuves en faveur de la nécessité de reviser le jugement de l'illustre chimiste, on le trouverait dans les faits suivants : il y a du fluor dans le bile, il y en a dans l'albumine de l'œuf, il y a dans la gélatine, il y en a dans la salive, dans l'urine, dans les cheveux ; il y en a dans les poils d'animaux (bœuf, vache, et veau) ; en un mot, l'organisme est pénétré de fluor ; on peut s'attendre à en trouver dans tous les liquides qui l'imprègnent.

" Dans un prochain travail je ferai connaître les procédés très simples à l'aide desquels j'ai pu reconnaître la présence du fluor dans toutes ces matières. Pour le moment, je dois me borner à prendre date et à prier l'Académie de me donner acte de cette communication."

I subjoin, for convenience of reference, an English translation :—

" From considerations which I shall shortly have the honour to submit to the Academy, I have been led to verify the much disputed assertion of the presence of fluorine in the bones. My experiments having been affirmative, I sought for fluorine in the blood, the only channel by which it could have reached the osseous tissue ; and I found notable quantities of it, not only in human blood, but also in that of several of the Mammalia (pig, sheep, ox, dog) ; of several birds (turkey, goose, duck, fowl).

" Results so uniform appear to me to give to fluorine an importance which it has not yet obtained in medicine or physiology ; they manifestly contradict the opinion of Berzelius, that the presence of fluorine in the bones is purely accidental, and that it is at any rate non-essential.

" If other proofs were needed to show the necessity of revising the judgment of the illustrious chemist, they would be found in the following facts : there is fluorine in the bile, in the albumen of eggs, in the saliva, in the urine, in the hair ; in the hairs of animals (ox, cow, calf) ; in a word, the organism is penetrated by fluorine, and we may expect to find some in all the liquids with which it is impregnated.

In an early work I shall make known the very simple processes by means of which I have recognised the presence of fluorine in all those substances. For the present I limit myself to noting the date, and asking the Academy to give me formal acknowledgment of this communication."

From the statement of Nicklès, which I have quoted in full, it will be seen that its author was led by his verification of the conclusion, first announced at Rome by Morichini and Gay-Lussac in 1802, that fluorine occurs in the bones of animals, to infer that it must be conveyed to these organs by the blood, and to seek for it in that fluid.

The majority of analysts, however, have long ago justified the early Roman observations. In particular the question of the presence of fluorine in bones was keenly contested in London in 1843, and analyses confirmatory of its occurrence in them were published by Professor Daubeny and Mr Middleton; to which in 1846 I added, in a communication made to this Society, the accordant results obtained by Professor Gregory and myself, and drew attention to the suggestion of Professor Graham of London, and of Dana, the American geologist, that animals possibly derived the fluorine found in their tissues from fluoride of calcium held in solution by water containing carbonic acid. In the same paper I adverted to the conclusion of Mr Middleton, founded on his detection of fluorine in a multitude of aqueous deposits, that "beyond a doubt it is present in water, though perhaps in very minute quantity. . . . . The simple fact that the blood conveys it to the bones would, I apprehend, sufficiently confute any scepticism on the subject."

At this point I took up the inquiry in January 1846, and on April 6 of that year communicated a paper to this Society, in the "Transactions" of which it was published. It will be sufficient here to give an epitome of its contents. The paper was entitled "On the Solubility of Fluoride of Calcium in Water, and its relation to the occurrence of Fluorine in Minerals, and in Recent and Fossil Plants and Animals." It is divided into seven sections. The first, entitled "Introductory Remarks," details the researches of my predecessors, including those to which I have just referred. The second, entitled "Of the Solubility of Fluoride of Calcium in Water," points out, that, contrary to previous belief, this salt is dissolved by pure water, yielding a solution answering to all the tests of lime and of hydrofluoric acid. The third, entitled "Of the presence of Fluorine in Well, River, and Sea Water," confirms and extends the observations of previous analysts on the occurrence of a dissolved fluoride in fresh water, and for the first time announces its direct discovery in sea-water, where Middleton and Dana had independently anticipated its presence, after finding it invariably in the shells of marine mollusca and in corals. The fourth section, entitled " Of the presence of Fluorine in Minerals," does not call for special notice. The fifth entitled " Of the presence of Fluorine in Plants," confirms the results of Will of Giessen as to the existence of this element in the ashes of vegetables, and draws attention to plants and to water as the media by which fluorides may be transferred from the soil to animals. The sixth section, entitled " Of the presence of Fluorine in Animals," commences with the statement, " As there exists, then, a twofold source of fluorine for animals, we may anticipate its occurrence in various parts of their structure;" and thereafter announces, in opposition to the negative results of Dr Rees, my confirmation of the observation of Berzelius, that a fluoride is present in human urine,—a result which the great Swedish chemist hailed with satisfaction before his death*, although M. Nicklès seems to think that he has been the first to confirm the original assertion. The paper then proceeds to state,—" It could not be doubted, after the facts I have detailed, that fluorine would be found in the two great formative liquids of the animal body, blood and milk; I have found it in both. So far as I am aware, it has hitherto been overlooked in all the analyses that have been made of these liquids; probably it has not been sought for. I employed the blood of the ox, and in two cases obtained markings on glass which only became visible when breathed upon, but are then quite manifest. In the third, the glass was distinctly, though faintly, corroded."

The concluding part of this section is occupied with a criticism of the declaration of Treviranus, that the gastric juice of birds contains hydrofluoric acid ; the final sentence being, " We may now look for fluorine in all the animal fluids."

I merely name the title of the seventh section, which is headed " Of the presence of Fluorine in Fossil Bones, and its relation to Animal Life."

In the summer of the same year, 1846, I ascertained the extent to which pure water dissolves fluor-spar, namely, 0·26 grains in 7000 grains of the liquid at 60° F. This result was announced to the British Association at its meeting for that year, and to this Society in November. In 1849 these observations were repeated with certain variations, to meet objections which had been raised to my conclusions, but with the same result. In 1849 I communicated to the British Association the results of a series of analyses, demonstrating by a new method of inquiry the presence of fluorine in the waters of the Frith of Forth, the Frith of Clyde, and the German Ocean; and in March 1850 I communicated to this Society an additional series of observations made in the same way, but extended to the waters of the Irish Sea, of the Atlantic, and the Mediterranean. This paper was accompanied by a letter from Professor Forchammer of Copenhagen, testifying to the presence of fluorine in the waters of the Baltic. In the summer of the same year (1850) I returned to the analysis of blood and milk for fluorine, feeling assured that still more decisive proofs of its presence in both could be obtained by using a larger amount of material, and subjecting it to a simpler process. Accordingly, employing in the case of blood (which was that of the ox) 26 imperial pints, in the case of milk 9 imperial pints, and in that of cheese 12 lbs., I was able to etch glasses with the hydrofluoric acid evolved from them so deeply that they might have been printed from, like copper plates. The etched glasses were shown to the members of the Chemical and Physiological Sections of the British Association, at its meeting in Edinburgh in 1850, and the details of the process published in its "Transactions," as well as in the Edinburgh Philosophical Journal for October of that year.* In the spring of 1852, I again brought the subject before this Society in a paper entitled "On two new Processes for the Detection of Fluorine when accompanied by Silica; and on the presence of Fluorine in Granite, Trap, and other Igneous Rocks, and in the Ashes of Recent and Fossil Plants." (Read April 19, 1852.) In the summer also of the same year, a communication, founded on an application of these processes, was made to the Botanical Society of Edinburgh,—"On the presence of Fluorine in the stems of Gramineæ, Equisetaceæ, and other Plants, with observations on the sources from which vegetables derive this element." In this communication, read July 8, 1852, I reported the results of an examination of twenty-four plants or vegetable products, in twelve of which fluorine was found. It will suffice to state in reference to both papers, that their object is to point out, and illustrate by examples, methods of readily discovering fluorine in circumstances which previously rendered its detection difficult.

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* *Jahres-Bericht*, von Jacob Berzelius, 1848, p. 164, which contains a general comment on my researches of 1846.

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* They are specially referred to in the English translation of Lehmann's "Physiological Chemistry," by Prof. G. E. Day, vol. i., p. 425. Cav. Soc. Publ. 1851.

The more perfect of the two processes has been applied with success by Professor Hoffmann to the detection of fluorine in the mineral waters of Harrogate; and Fresenius has introduced it into the last edition of his "Qualitative Analysis."*

The researches thus referred to have been chiefly published in the "Transactions" of this Society, and of the British Association, but have been brought in part before the Chemical Society of London. They are known in Germany, Denmark, Sweden, and America, and have been referred to by many authors in this country. It is reasonable, accordingly, to infer that some knowledge of them has reached Paris; and it might have been supposed that they had not altogether escaped the notice of M. Nicklès, whose name appears on the title-page of the *Journal de Pharmacie et de Chimie*, as editing the department of that work entitled "Une revue des Travaux Chimiques publiés à l'Etranger."

I bring no charge, however, against M. Nicklès. In these days of multiplied monographs it would be unjust to blame any man for ignorance of a single series of special researches. Nevertheless, seeing that this author's name appears on the title-page of the *Journal de Pharmacie* side by side with those of our Vice-President Dr Christison, as its Edinburgh Correspondent, and of Dr Redwood, the Secretary of the Cavendish Society, as its London Correspondent, the countrymen of M. Nicklès, may think themselves entitled to quote the legal maxim, "de non apparentibus et de non existentibus eadem ratio," and to infer that what of reputed English science is not known to him, does not exist to be known. Whilst, therefore, I wish M. Nicklès all success in extending our knowledge of the organismal distribution of fluorine, I ask from him, now that he is made aware of the fact, acknowledgment of my priority in reference to the discovery which he specially claims, and of the other discoveries which the papers referred to announce.

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* Fourth edition of the English translation, 1855, p. 134, stated by its editor, Mr J. L. Bullock, to correspond with the eighth German edition. The process essentially consists in heating the silicated fluoride with oil of vitriol, and condensing the gaseous fluoride of silicon in aqueous ammonia, which after evaporation, re-solution in water, and desiccation, yields fluoride of ammonium. Fresenius recommends the addition of "some coarse pieces of marble to insure a continuous slight evolution of gas;" but I cannot approve of this recommendation, since the constant occurence of fluorine in shells and corals implies its presence in limestones; and the employment of marble for the purpose indicated risks the introduction of the very element for which we are seeking.

The following Gentlemen were admitted Ordinary Fellows:—

Right Hon. John Melville, Lord Provost.
John Blackwood, Esq., 3 Randolph Crescent.
Brinsley de Courcy Nixon, Esq., London.

The following Donations to the Library were announced:—

Monograph of the genus Abrothallus. By W. Lauder Lindsay, M.D., 8vo.—*From the Author.*

Reports of the Meetings of the Royal Institute of British Architects.—*From the Institute.*

Meteorological Observations taken during the years 1829 to 1852, at the Ordnance Survey Office, Phœnix Park, Dublin; to which is added a series of similar observations made at the principal trigonometrical stations, &c., in Ireland. Edited by Capt. Cameron, R.E.; Lieut.-Col. H. James, R.E., Superintendent of Survey, 4to.—*From the Right Hon. The Secretary of State for War.*

Proceedings of the Royal Astronomical Society, XVII., No. 23, 8vo.—*From the Society.*

Memoires de la Société Impériale des Sciences Naturelles de Cherbourg. Vol. III. (1855) 8vo.—*From the Society.*

British Interests in the Canalization of the Isthmus of Suez. 8vo. —*From the Author.*

Papers read at the Royal Institute of British Architects, January 1857. 4to.—*From the Institute.*

Tables showing the number of Criminal Offenders in England and Wales in the year 1855. Folio.—*From the Right Hon. the Secretary of State.*

Instructions for making Meteorological Observations.—*From the Scottish Meteorological Society.*

Monthly Returns of the Births, Deaths, and Marriages, registered in the eight principal towns of Scotland; with the causes of death at four periods of life.—*From the Registrar-General.*

Quarterly Returns of the Births, Deaths, and Marriages, registered in the Divisions, Counties, and Districts of Scotland.—*From the Registrar-General.*

Monday, March 2, 1857.

The Right Rev. BISHOP TERROT, Vice-President, in the Chair.

The following Communications were read:—

1. On the Functions of the Spinal Cord. By Professor Hughes Bennett.

The object of Dr Bennett's communication was to unite two separate kinds of research, which of late had been directed towards advancing our knowledge of the structure and functions of the spinal cord. From these it would, he thought, appear, that the views considered to be so firmly established by the genius and labours of Charles Bell, required great modification. Dr Bennett then gave a sketch of these views, and of the present opinions of physiologists regarding the functions of the spinal cord. He indicated certain facts which had long been recognised as difficult of explanation in accordance with them. He then described the results of several experiments by M. Brown-Séquard on the columns of the cord in living animals, which he himself (Dr B.) had witnessed, and which satisfied him that, on the posterior columns being cut across, increase of sensibility in the inferior extremities was the consequence, instead of paralysis. He also described the discoveries recently made in the structure of the spinal cord, by Budge, Kölliker, Lockhart Clarke, Stilling, Remack, Wagner, Van der Kolk, Schiling, Kupffner, and especially by Owsjannikow. He pointed out how the structural discoveries threw light on the experimental ones, and from the whole inquiry drew the following conclusions:—

1. Although the anterior and posterior roots of the spinal nerves may still be considered motor and sensitive, we can no longer apply these terms to the anterior and posterior columns of the cord.

2. The fibres in these columns do not convey impressions directly and continuously to the brain as hitherto supposed, but enter the grey matter, and operate through the ganglionic cells of that matter.

3. That all so-called reflex movements are carried on by a definite system of conducting fibres and ganglionic cells, passing through the grey matter; in other words, they are *diastaltic* and not reflex.

4. That the particular fibres and cells which are necessary to spinal diastaltic acts have yet to be discovered; so that a new field of inquiry is opened up to the physiological histologist.

2. On the Delta of the Irrawaddy. By T. Login, C.E., Pegu. Communicated by William Swan, Esq.

Little is known of the course of the Irrawaddy River above Ava, and as it is joined by no large tributary near its mouth, its sectional area differs very little for hundreds of miles. At Prome, a distance of 190 miles in a direct line from the sea, the river is confined between two ranges of hills. The eastern range passes through the Tharawaddy district, separates the Irrawaddy from the Sittang Valley, and is lost in low undulating hills at Rangoon. The western or Aracan range is more mountainous, and terminates in a bluff headland forming the right bank of the Bassein River at its mouth. The plain bounded by these two ranges is inundated when the river is in flood—so much so, that it can be traversed in almost any direction in small canoes.

The Irrawaddy, like all other large rivers in India, begins to rise in March, and attains its highest elevation in August, after which it gradually subsides, until it is again swollen by the melting of the snows among the hills, when it rises from 10 to 12 feet before the setting in of the rains.

My observations were made at Than-ba-ya-doing in March 1855, with as great care as circumstances would permit, to determine the discharge, velocity, slope, and the proportions of earthy matter suspended in the water. It was found that, when the river was at its lowest, it discharged 75,000 cubic feet per second. The mean surface velocity was $1\frac{1}{2}$ miles in the hour, the slope $1\frac{3}{5}$ inches in the mile, and the proportion of earthy matter was $\frac{1}{5725}$ by weight.

When the river is in flood its surface rises 37 feet higher than in the dry season; the discharge was estimated at 750,000 cubic feet in the second, with a surface velocity of 5 miles in the hour, a slope of $3\frac{4}{5}$ inches in the mile, and the proportion of silt about $\frac{1}{1700}$ by weight.

Although, however, I had thus obtained the extreme cases approximately, I had no means of determining the average discharge for the 12 months; but for my present purpose, I have supposed it to be 350,000 cubic feet per second, containing $\frac{1}{3000}$ part by weight of earthy matter; and as the clay of the delta is nearly twice the specific gravity of water, there would be 60 cubic feet of silt passing Than-ba-ya-doing every second, to form the delta; or say 2,000,000,000 cubic feet annually.

The apex of the delta, and the mouths of the Rangoon and Bassein Rivers, are each about 140 miles apart, which would give an area of 8500 square miles; but as the delta is intersected by creeks, the dry land may be 7500 square miles. For the sake of calculation, I have supposed the silt to be evenly deposited to a distance of 25 miles out at sea off the mouths of the river, and that each annual stratum is five times the thickness of the deposits on the dry land of the delta. By the above approximate data, which I trust will be found hereafter to differ not very widely from the truth, the following is obtained:—

$$\left(140 \times 25 + \frac{7500}{5}\right) \times (5280)^2 = 139{,}392{,}000{,}000 \text{ square feet;}$$

or say an area of 140,000,000,000 square feet to be covered by 2,000,000,000 cubic feet of silt: thus, each cubic foot would have to cover 70 square feet at sea, and 350 square feet on the delta, or the sea would become $\frac{1}{6}$th of an inch shallower every year, and the land would be raised $\frac{1}{30}$th of an inch. The delta, however, must be 6 feet higher at the Rangoon mouth of the river than at the Bassein mouth, as the tide rises 21 feet at the former point and only 9 feet at the latter. Again, as correctly as I could learn, the tidal wave is not sensibly felt higher up than 35 miles below Than-ba-ya-doing in the dry season; and, in the rains, it only reaches a point 95 miles lower down, as measured along the course of the stream. I could thus approximately find the level of Than-ba-ya-doing to be 40 feet above high tide, and the slope of the river $3\frac{4}{5}$ inches.

Supposing, therefore, my calculations to be correct, and not taking into account the effects of vegetation, it must have taken 14,400 years to raise the head of the delta above high tide to its present level. If to this be added the probable time it took to raise the beds of the ocean above high water, even with such a powerful agent at work as the Irrawaddy, ages on ages must have elapsed since this silting up process began. From the great depths of the river, and its liability to change its course, relics of man, and bones of existing animals, may hereafter be found even more than 100 feet below the sea, at different points throughout the delta. Caution should therefore be observed in ascribing antiquity to such relics, nor should they be considered a proof of the subsidence of the land.

Before commencing this survey, Lieutenant Walker of the Bengal Engineers pointed out to me the inaccuracy of M. Du Buat's rule for calculating the discharge of rivers. I also, while engaged on the survey, discovered that the fundamental rule for finding the mean and bottom velocities, by the known surface velocity, was also inaccurate. The rule is, where the surface velocity v is expressed in inches, the bottom velocity equals $(\sqrt{v}-1)^2$.

But even when the river was at its lowest, the bed, which consisted of sand, could only have withstood half the velocity calculated by this rule ; and when the river was in flood, even large boulders would have been swept along by the current. To this fact I beg to draw particular attention, for at no point did I find the bed to consist of such materials as could withstand the calculated velocity ; but the nature of the bed always varied according to the *depth* and surface velocity. I therefore estimated the bottom velocity by the nature of the bed, instead of abiding by the above rule, and found the mean velocity, by halving the sum of the estimated bottom and known surface velocities. Mr Ellet, however, found on the Mississippi, that where the river was deep, the velocity was always greater at some depth below the surface than at the surface itself.

Another source of error,—the one of all others most difficult for the engineer to contend against,—is the power rivers have of abrading their beds to considerable depths, and again silting them up to their former level while the flood is subsiding.

I have had many opportunities of observing this process of scooping out the bed and again silting it up, and I have known it to extend to a depth of ten and a-half feet in rear of one of the works on the Ganges Canal that was under my orders.

In the accompanying table I have compared the Irrawaddy with a few of the largest rivers. (P. 476.)

The discharge of the Ganges appears to me much too small; for I have seen both the Nile and the Ganges in flood, and should say that the Ganges at Gazepoor is nearly three times the size of the Nile. The proportions of earthy matter vary to a great extent in all the above-mentioned rivers. The Ganges appears to convey the largest proportion of silt and the Rhine the least.

The power of a river in transporting earthy matter, however, chiefly depends on the *shape* of the particles: sand, which is only one-seventh the specific gravity of gold, will sink much faster in water than gold leaf. By investigating this branch of the subject, the power of water in motion to transport solid bodies may be discovered, as the rate of sinking of any solid body must bear some proportion to the velocity of the water required to transport that body. It may here be also remarked, that where there is a strong current in the ocean, rivers cannot push out deltas into the sea; and rivers which fall into tideless seas have no trumpet-shaped mouths, nor are they easily navigated, for even the Mississippi has only fourteen feet water at its mouth, though it is 170 feet deep higher up the river, and the bar consists of soft mud.

As a navigable stream the Irrawaddy is second to few rivers in the world; for not only are its mouths easily approached, but for hundreds of miles up its course it has been found much more navigable than the Ganges. The valley through which this noble river flows equals, if it does not surpass Bengal in the richness of its soil; can it be doubted, therefore, but that ere long Anglo-Saxon enterprise, and civilization, will force its way into the interior of this rich country?

P.S.—On this paper being read, Professor Forbes drew the attention of the Society to that part which related to the abrading power of water at different velocities; he stated that the experiments referred to were made fifty years ago by Professor Robison, his predecessor; and as the subject was of considerable interest, he hoped some one would verify these experiments.

Through his kindness in giving me the use of some of his instruments and class-room, along with the assistance of one of his establishment, I am enabled to give the few following results of experiments tried on brick-clay from Portobello, sea and fresh-water sand, rounded pebbles about the size of peas, and common vegetable soil.

1. The brick clay, in its natural moist state, had a specific gravity of 2·05; and water passing over it for half an hour at a rate of 128 feet in the minute, which was the greatest velocity I could conveniently obtain, made no visible impression on the clay. When this clay was mixed with water, and allowed to settle for half an hour, it required a velocity of fifteen feet in the minute to disturb it. This mud sank in water at a rate of 0·566 feet in one minute, but the very fine particles were very much longer in subsiding.

2. The fresh-water sand, which sank in water at an average rate of 10 feet in the minute, required a velocity of 40 feet in the minute over the bed to disturb it.

3. The sea-sand sank 11·707 feet in one minute, and was moved over the bed by a velocity of 66·22 feet per minute.

4. The rounded pebbles, about the size of peas, which sank at the rate of 60 feet in one minute, were rolled over each other by a velocity of 120 feet in one minute.

5. The vegetable soil, being a mixture of different kinds of particles, it was difficult to determine the rate of sinking. The very fine particles were swept away by a velocity of 2·45 feet in the minute, and those that were disturbed by a velocity of 33⅓ feet in the minute, sank at a rate of 0·98 feet per minute only. All the vegetable soil was swept away when the velocity of the water over the bed was increased to 50 feet in the minute, and the particles of sand left, of which nearly one-half of this soil consisted, were set in motion by this velocity. This sand, however, only sank at a rate of 5·62 feet in one minute.

From the above it is evident that the velocity of water over the beds of canals, and water-courses for irrigation, unless protected by a pebbly bed, should never exceed half-a-mile an hour, otherwise the fields irrigated will be covered with a stratum of sand.

These experiments were made with water seldom exceeding half an inch in depth, and the float was within ¼ or ⅛ of an inch of the bottom. The time was measured by an instrument which indicated tenths of seconds.

Comparison of the Irrawaddy with other large Rivers.

NAME OF RIVER.	Discharge per second in cubic feet.			Proportions of Silt by weight.			Slope per Mile in Inches.	Surface velocity in Miles per hour.		
	Greatest.	Least.	Mean.	Largest	Smallest.	Mean.		In Flood.	In Dry Season.	
Irrawaddy, .	750,000	75,000	350,000	$\frac{1}{1700}$	$\frac{1}{5725}$	$\frac{1}{3000}$	3·80	3½ to 5	1½	Mr Login.
Mississippi, .	1,700,000	...	385,000	$\frac{1}{693}$	$\frac{1}{2310}$	$\frac{1}{1155}$	3·25	5 to 7	...	Mr Ellet.
Ganges at Gazepoor, .	494,208	36,330	207,000	...	...	$\frac{1}{428}$	4·00	4 to 7	...	The Rev. Mr Everest.
Burrumpooter,	...	150,000	...	...	...	...	...	...	...	Measured in January by Major Wilcox.
Nile, . . .	333,394	214,621	101,000	...	...	$\frac{1}{633}$	5 to 5½	3¾	1¾	Mr Horner.
Rhine at Bonn,	...	...	66,000	$\frac{1}{12500}$	$\frac{1}{2073}$	$\frac{1}{16340}$	...	...	...	Ditto.
Amazon, . .	...	...	...	...	...	...	2·35	3½	...	

3. Notice of a Collection of Maps. By A. K. Johnston, Esq.

In this paper the author reported the progress made by the committee appointed to select and purchase a series of chartagraphic works for the library of the Royal Society, and the means adopted for their arrangement and classification. The collection already comprises 534 separate sheets of the best existing maps, chiefly of the several countries of Europe, but embracing the survey of India, in so far as published. The maps are placed in cases resembling volumes, so indexed as to admit of being indefinitely extended, and easily consulted. Specimens of the different works were exhibited, and the author presented a rapid sketch of the progress of surveying and mapping, from the sixteenth century to the present time. He showed that modern improvement in this important branch of science dates from the middle of the 18th century, when, in 1750, Cassini de Thury, under the auspices of the French Academy of Sciences, constructed a map of France on astronomical principles. In 1784 the French triangulation was extended to London, and formed the basis of the trigonometrical survey of Great Britain. The surveys of Belgium, the Netherlands, Prussia, and Sardinia, have also been based on that of France. The different methods adopted to represent relief of the surface by contour lines and hill shading, were then referred to, and examples of the effects produced by vertical and oblique lights were exhibited. It was shown that the method which supposes the light to fall vertically on the model, casting the shadow in all directions, gives the most exact idea of the inequalities of the ground, and that it is adopted in nearly all the great survey maps now in progress. As an example of the time and labour necessary to produce a good map, it was explained that in the great survey of France, now nearly completed, a single sheet requires, for reduction and drawing, at least two years, and for engraving, five to eight years. Thus, between the termination of the field-work of the surveyor and the publication, seven to ten years must necessarily elapse. Mr Keith Johnston concluded his remarks by referring to the economical advantages of the electrotype process in reproducing copies of original plates, thus reducing the price of the publication; and to an ingenious application of this process, recently adopted at the Depôt de la Guerre, Paris, by which erasures made in the work, for correction, are filled up by a fresh deposit of copper, leaving a surface ready for being re-engraved.

COLLECTION OF MAPS IN CASES.

EUROPE.

Title.	Author.	Sheets.
Europe (Geological),	Murchison,	4
British Isles (Geological),	Knipe,	4
England and Wales (Geological),	Government Survey,	104*
Ireland,	Ordnance Office,	6
England and Wales, Topographical,	Ordnance Survey,	65*
Scandinavia,	Forsell,	9
N. Deutschland,	Englehardt,	24
Deutschland, Belg., Schweiz,	Stieler,	25
Königreich Sachsen,	State Survey,	15*
Kurhessen (Geological),	Schwarzenberg,	1
Thüringer Waldes (Geological),	Credner,	4
Salzburg,	General Staab,	1
Würtemberg,	Bach,	5
Belgique (La) (Geological),	Dumont,	10
France (Topographical),	Etat-Major,	14*
France (Geological),	E. de Beaumont,	6
Schweiz, Topographical,	Dufour,	17*
Suisse (Geological),	Studer,	4
Russie d'Europe,	Dep. de la Guerre,	29*
Oesterreichische Kaiserthum,	Fallon,	9
Lombardo-Veneto (Regno),	Austr. Survey,	4
Stati di S. M. Sarda,	Survey,	6
Environs de Rome,	Dep. de la Guerre,	1*
Alpes, Piemont, Savoye, &c.,	Raymond,	13
Stato Pontificio e G. D. Toscana,	Austr. Survey,	52
Mont Blanc,	Raymond,	1
Kaukasischen Isthmus,	Koch,	4
Türkischen Reiches in Eur.,	Kiepert,	4
Thrace,	Viquesnel,	1
Grêce,	Dep. de la Guerre,	20
Islande,	Olsen,	4

ASIA.

India, (Geological),	Greenough,	12
India, E. I. C.,	Government Survey,	51*
Klein Asien,	Kiepert,	16

Those marked with an asterisk thus, * are incomplete, the works being only in progress.

Collection of Maps and Charts.—Continued.

Pilote Française. Dépôt Général de la Marine. 12 tomes.

Third part of the General Survey of England and Wales, containing Cornwall. By Colonel Mudge. (Ordnance.)

The National Atlas of Historical, Commercial, and Political Geography. By A. Keith Johnston, F.R.S.E., F.R.G.S., &c.

The Physical Atlas of Natural Phenomena. By A. Keith Johnston, F.R.S.E., F.R.G.S., &c.

A Geological Map of Scotland. (On roller.) By Dr M'Culloch, F.R.S.

Ordnance Survey of Ireland; scale, six inches to a mile. Twenty-four volumes.

Geological Map of England and Wales. (On roller) By G. B. Greenough.

Atlas, containing Maps of Poland, exhibiting the political changes from 1772 to 1837. By J. M. Bansemer and P. F. Zaleski.

Charts, &c., published by the Hydrographic Office of the Admiralty, London. These are arranged in trays, in the Museum.

The Counties of Perth and Clackmannan. (On roller.) By James Stobie. 1805.

Various Sailing Charts, &c., of the United States.

The following Gentlemen were elected Ordinary Fellows:—

Andrew Murray, Esq. of Conland, W.S.
Rev. Dr Macfarlane, Duddingston.
Dr W. M. Buchanan, E.I.C.S.

Monday, March 16, 1857.

Dr CHRISTISON, V.P., in the Chair.

The following Communications were read :—

1. Notice respecting Father Secchi's Statical Barometer, and on the Origin of the Cathetometer. By Professor Forbes.

A friend, who returned lately from Rome, has sent me some copies of a pamphlet by Father Secchi of the Collegio Romano, one of which I lay on the table of the Society.

It describes a barometer stated to be on a new construction. The barometric tube is suspended from one arm of a balance, and counterpoised. It is filled with mercury in the usual way; but the cistern into which it opens is fixed apart, and does not move with the beam of the balance. It is evident, therefore, that the varying pressure of the air on the exterior of the tube will require a changing counterpoise, and that the magnitude of the change may be increased by enlarging the section of the tube, so that the alteration of pressure may be indicated with any required delicacy.

It is also obvious that, to use this barometer, the tube does not require to be transparent, but may, for instance, be made of iron; only the internal section must be uniform throughout the range of pressure.

The idea of thus measuring barometric pressures appears so obvious that it is not likely to be really new. But I had also, when I read the paper, a distinct recollection of having seen it described many years ago.

After a slight search I found it, accordingly, under the name of the *Steelyard Barometer* (the tube being suspended from the shorter arm of a steelyard, while the other points to the angular deviation on a scale), in Rees, and others of the older Encyclopædias (as in the earlier editions of the *Britannica*), in *Hutton's Mathematical Dictionary*, and in *Gehler's Wörterbuch*. But, what is singular, no inventor is assigned to the contrivance, except in the last-named work, where it is described generally as Morland's; though Hutton, who is there cited as the authority, says nothing of it.

In *Desagulier's Natural Philosophy* (1763), an experiment with a balance, similar to Father Secchi's arrangement, is described and figured, but it is not referred to as a construction available for practical purposes. This might lead one to believe that the contrivance was more recent than Desagulier's time. But, after considerable search, I found, in the nineteenth volume of *Rozier's Observations de Physique* (1782), page 346, a curious historical statement by Magellan, which refers the contrivance to Sir Samuel Morland, who, it is there stated, presented it to Charles II. Magellan does not, however, give his authority for this, stating, on the contrary, that he found no mention of the contrivance in any of the authors who had treated of the subject, but that he had seen two of these instruments. One of them, made by Adams in 1760, belonged to George III.; and I think it possible that it may still be found amongst the instruments of the Kew Observatory. The other was made by the celebrated Sisson, and came into M. Magellan's possession; a careful figure of it is given in the work just cited. It is perhaps likely that the ascription of it to Morland, and the story of its presentation to Charles II., was a tradition among the London instrument-makers. It may, however, be recorded in some of Sir Samuel Morland's writings, which I have not found either in the College or the Advocates' Library, and in which it does not appear that Magellan had himself seen it.

I have as yet been unable to trace the steelyard modification of the statical barometer to its origin. I think it likely to be an independent invention.

Of course these remarks are not intended to infer the smallest doubt on Father Secchi being the inventor of the instrument which he describes. Of that there can be no question; and the application of it, which Father Secchi proposes, to the purposes of self-registration, makes it a well-timed resuscitation of an almost forgotten contrivance, which yet appears to date from the same century with the invention of the barometer.

2d March 1857.

Postscript—16*th March* 1857.—I have not succeeded in throwing any further light on the true origin of the statical barometer. On writing to Mr Welsh of the Kew Observatory, I find that King George III.'s curious collection of apparatus has been long dispersed.

I ought perhaps to add, with reference to Father Secchi's contrivance, that he recommends in some cases the *cistern* of the barometer to be made moveable, instead of the tube. The balance is then disturbed by the efflux of mercury from the tube of the barometer when the pressure diminishes, and by its influx when the pressure increases. Though less elegant, as an application of a principle, it has the advantage of making the suspended mass lighter. It will be seen, by a reference to Magellan's account of Sisson's instrument, that the weight was such as to require support on friction-rollers, instead of knife edges.*

Invention of the Cathetometer.—I take this opportunity of adding a historical notice, which has occurred to me whilst making the preceding inquiry. In the twentieth volume of the Philosophical Transactions for 1698, Mr Stephen Gray described a microscope moving on a vertical pillar by means of a micrometer screw, to be used for determining the exact variations of level of a liquid, such as mercury in a barometer or thermometer, and not necessarily connected with the apparatus. This instrument accurately corresponds in most respects with that known to French physicists and instrument-makers under the name of the *Cathetomètre*, which I have never heard ascribed to any inventor in particular, and which, till very lately, has hardly been recognised in this country.

2. History of an Anencephalic Child. By Dr Simpson.

3. On certain Laws observed in the Mutual Action of Sulphuric Acid and Water. By Balfour Stewart, Esq. Communicated by Dr G. Wilson.

The object of this paper was to show that where sulphuric acid combines with water, distinct reference is made to certain definite compounds or hydrates of sulphuric acid.

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* Since this paper was read, I have been enabled to carry back the history of the Balance Barometer, or at least of the experiment described by Desaguliers, considerably farther. In Cotes's *Lectures on Hydrostatics*, &c., published by Smith in 1747 (but which were delivered more than forty years previously), the experiment is fully detailed and explained. It is also ascribed to Wallis, as well as an ingenious modification of it well adapted for the lecture table.

April 1857. J. D. F.

The combination of these two liquids is attended with contraction of volume; that is, the volume occupied by the compound is less than the sum of the volumes occupied by its ingredients when uncombined. By means of a simple formula (assuming 1·8485 to be the specific gravity of strong liquid sulphuric acid), we may find what ought to be the specific gravities of the different strengths in Dr Ure's table, were no contraction to take place. By this table we may find the actual specific gravities of such mixtures; and dividing the actual or observed specific gravity by the calculated specific gravity, and deducting unity from the quotient, we have the proportional condensation.

The proportional condensation is greatest for strength 73 of Dr Ure's table, which is the strength of a hydrate composed of one atom of liquid acid and two atoms of water.

But it is not necessary to suppose all the strengths of Dr Ure's table to be formed by mixing together strong liquid acid and water; for, taking a certain strength as our standard, we may suppose all mixtures stronger than it to be formed by mixing it with strong acid, and all mixtures weaker than it to be formed by mixing it with water in certain proportions.

On this hypothesis we shall have different calculated specific gravities, and consequently, different proportional condensations from those obtained when all strengths were viewed as composed of strong acid and water.

It was shown that a great range of standard strengths gives a maximum at 73, as before, while others indicate a maximum between 84 and 85, denoting a hydrate composed of one atom of liquid acid and one of water. These results were made visible by a curve, of which the abcissæ represented strengths, and the ordinates proportional condensations, and it was shown that points of greatest elevation or depression, or more generally peculiarities in the curve, denoted definite compounds of acid and water. By means of such a curve the following hydrates may be indicated, in addition to those already mentioned:—$SO_3HO + 5HO$, $SO_3HO + 7HO$, $SO_3HO + 8HO$, $SO_3HO + 11HO$, $SO_3HO + 12HO$, and $SO_3HO + 15HO$.

Independent experiments were made in order to see how far Dr Ure's observations were reliable; and a remarkable agreement was found for the weaker strengths tried; but in the higher strengths the observations seemed to show a constant error in Dr Ure's results, which make the specific gravities too low. The following is a list of the strengths tried, and of the corresponding specific gravities observed:—

Strength.	Observed Specific Gravity.
88·6	1·8041
48·0	1·3737
47·5	1·3688
47·0	1·3643
45·8	1·3537
28·0	1·2033
27·0	1·1954
26·6	1·1925
26·0	1·1874
25·0	1·1795
21·0	1·1481
20·0	1·1405
19·0	1·1329

The constant error supposed to pervade Dr Ure's determinations of specific gravities for the higher strengths, was accounted for by supposing that Dr Ure must have operated with two or more different specimens of acid; the error arising in his determination by chemical analysis of the strength of each, and different acids being used for high and low strengths. As an instance of this, taking strength 90 as our standard, the proportional condensations for strengths 68, 67, 66, 65, are respectively ·0426, ·0429, ·0410, ·0411; that for strength 66 being very much less than that for strength 67. This is indicated by an abrupt fall in the curve at that point, after which it goes on slowly rising, just as before its fall.

These experiments confirmed a maximum point corresponding to the hydrate $HO, SO_3 + 15\,HO$, and showed a minimum point corresponding to the hydrate $HO, SO_3 + 6\,HO$.

Allusion was made to Professor Langberg, who, in a report to the British Association for 1847, has described a method of research somewhat similar, but giving negative results. Professor Langberg expresses the specific gravity of an acid, in terms of its strength, by means of an empirical formula, the constants of which he derives from Dr Ure's experiments, and, by means of this formula, he is enabled to exhibit the proportional condensation of any strength (for a given standard) as a function of that strength, so that, equating the first differential coefficient of this function to zero, the resulting equation gives the position of maximum condensation. The points so determined do not correspond to definite compounds, probably because an empirical formula is used instead of the immediate results of experiment. In conclusion, the author's results were briefly stated thus :—

1. The points of elevation, depression, or peculiarity in the curve of condensation, denote definite compounds, whatever be the standard strength used.

2. The use of varying the standard is simply to render such points more prominent, or, in other words, to convert a point of peculiarity into one of elevation or depression, as the case may be.

The following Donations to the Library were announced :—

First Report of the Committee on Beneficent Institutions (Medical Charities of the Metropolis). 8vo.—*From the Statistical Society of London.*

Journal of the Statistical Society of London. Vol. XX., part 1. 8vo.—*From the Society.*

The Canadian Journal, January 1857.—*From the Canadian Institute.*

Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften. Mathematisch-naturwissenschaftlichen Classe, Band XX. heft 2 und 3; Band XXI. heft 1 und 2. Philosophisch-Historische Classe, Band XX. heft 2 und 3; Band XXI. heft 1 und 2. Register zu den zweiten 10 Banden. 8vo.—*From the Imperial Academy of Vienna.*

Denkschriften der Kaiserlichen Akademie der Wissenschaften, Wien. Philosophisch-Historische Classe, Siebenter Band. 4to.—*From the Imperial Academy of Vienna.*

Tageblatt der 32 versammlung Deutscher Naturforscher und Arzte in Wien im Jahre 1856. Nos. 1–8. 4to. *From the Imperial Academy of Vienna.*

Publications of the Königlisch Sachsische Gesellschaft der Wissenschaften, Leipzig, viz. :—

Beitrage zur Kenntniss der Gefässkryptogamen, von Wilhelm Hofmeister. 8vo.

Die Urkundlichen quellen zur Geschichte der Universität Leipzig in den ersten 150 jahren ihres bestehens, von Friedrich Zarncke. 8vo.

Elektrische Untersuchungen von W. G. Hankel. Erste Abhandlung, Uber die Messung der Atmosphärischen Elektricität nach absolutem maasse. 8vo.

—*From the Society.*

A General Index to the Philosophical Transactions, from the first to the end of the seventeenth volume. By Paul Henry Maty, M.A., F.R.S. 4to. London, 1787.—*From the University Library, Edinburgh.*

Supplement to the Quarterly Returns of the Births, Deaths, and Marriages registered in the Divisions and Counties of Scotland. Year 1856.—*From the Registrar-General.*

Monday, 6*th April* 1857.

DR CHRISTISON, V.P., in the Chair.

The following Communications were read:—

1. On the Structure of Pedicellina. By Prof Allman.

The author maintained that the genus Pedicellina, notwithstanding the circular arrangement of its tentacula, does not properly belong to the infundibulate Polyzoa at all, but is in reality hippocrepian, of which type, however, it presents a remarkable modification. The intestine at first sight appears to terminate within the margin of a orbicular lophophore, and, consequently, within the circle of tentacula, and thus to present a striking exception to the admitted plan of the Polyzoa. It was shown, however, that the anomaly which thus seems to exist was only apparent, for the lophophore, when carefully examined, is found to be constructed on the hippocrepian type, with the tentacula confined to the outer or convex margin, and the arms of the crescent united at their extremities so as to enclose a space, around which the tentacula will then be arranged in an uninterrupted circle, and within which the intestine opens, its termination being thus quite normal, and properly external to the lophophore.

As in the ordinary hippocrepian Polyzoa, so also here the mouth is furnished with an epistome, which, however, is less complete than in the others, and not provided with special muscles; and it is moreover highly probable that the calyx, which constitutes a universal feature in the ordinary hippocrepian genera, enters here into the composition of the peculiar cup which surrounds the base of the tentacula, and which the author believes has its homology in a permanently inverted portion of the endocyst, united externally to the uninverted endocyst, and internally to the calyx and tentacula.

2. On a Case of Lateral Refraction in the Island of Teneriffe. By Professor C. Piazzi Smyth.

In his astronomical visit to Teneriffe last summer, the author was instructed to inquire into the lateral oscillation of stars, as seen by Baron Von Humboldt in his ascent of the mountain. During a month's residence on the place of the alleged observation no approach to anything of the sort was ever noticed, although a powerful equatorial, with a twelve-foot telescope and high magnifying powers, was employed to detect any irregularity in the motions of the stars. The author concluded, therefore, that the anomalous movements described by Humboldt could not have been produced by any general or cosmical action of the atmosphere, or of light or heat, which astronomers were bound to consider.

3. On Insect Vision and Blind Insects. By Andrew Murray, Esq.

Mr Murray commences with a *resumé* of what is known regarding the growth of eyes in insects, from the first stage in the larva, when many are without eyes, till their exclusion from the chrysalis, when they usually appear well provided with compound eyes. He reviews the nervous system in different species, and gives some details as to those species which live in dark places, and which have small eyes and a less-developed optic nerve, contrary to what one would at first suppose. The next portion of his paper is devoted to explaining his views of the structure of the eye in insects, and its relation to the eye in vertebrate animals. Instead of seeking the homologies of the parts of the eye in the vertebrata in each separate eye tubule, or individual part of the compound eye in insects, as has hitherto been done, he compares it with the entire compound eye. Resting on the microscopic researches of Kölliker, H. Müller

Brucke, Hannover, Helmholtz, Goodsir, and others, into the intimate structure of the retina, he compares its structure with the structure of the compound eye in insects, making the filamentary layer equivalent to what is called by Leydig the retina in insects, the rods and cones in the bacillary layer to the conical bodies in the eye tubules of insects, &c. And he explains the discrepancy between the relative position of this structure in insects and in the vertebrata, on the principle suggested by Brucke and Hannover, and worked out by Goodsir, that the light is reflected back from the choroid or back of the eye in the vertebrate animal, so that the animal is, as it were, looking backwards, and sees objects as reflected in a mirror; while in insects, he assumes that objects impinge directly on their visual sensorium.

The rest of the paper is occupied with an examination of those insects which are destitute of eyes in their perfect state, with particulars relating to their habits and structure, and concludes with the results of his examination of the interior structure of the integument in the *Anophthalmus Bilimekii*, Schm., from which it appears that the interior texture of the thorax is a series of transverse elongate cells, similar to the cells in plants, and which is known to be the usual, if not the universal, structure of the chitonous integument in insects; the same cells are to be seen in the head; but on the ocular spaces where the eyes should have been, and which (in *Anophthalmus*) occupy a large portion of the head, these cells become enlarged, and gradually less transverse, until, towards the middle of the ocular space, some of them have assumed the hexagonal form usually seen in the facets of the compound eye in insects; whence, Mr Murray concluded, that this is possibly an atrophied or abortive eye, and draws conclusions as to the manner of the development of the eye in insects.

Mr Murray also considers the question, whether these insects are sensible of light, and if so, whether it is through this atrophied eye or not? He supposes they are, to a certain extent, sensible of light; but only in the same way as plants or zoophytes, and not through any optical apparatus.

4. On the mode in which Light acts on the Ultimate Nervous Structures of the Eye, and on the relations between Simple and Compound Eyes. By Professor Goodsir.

Since the publication in 1826, of Joh. Müller's *Vergleichende Physiologie des Gesichtssinnes*, Physiologists have admitted three fundamental forms of the organ of vision. 1*st*, The eye-spot, organized for the mere perception of light. 2*d*, The compound eye, in which the picture on the nervous surface is a mosaic. 3*d*, The simple eye, in which the retinal picture is continuous. The difference between the simple and compound eye, as explained by Müller, and since generally admitted, consists in this, that the formation of the picture in the simple eye is the result of the convergence of all the pencils diverging from the visible points of the object on corresponding points of the retina, by means of the crystalline lenticular structure of the organ; while, in the compound eye, the picture is formed by the stopping off, by means of the constituent crystalline columns of the eye, all rays except those which pass in or near the axes of the columns. The extent of surface of any object, and the number of separate parts of such surface, represented on the nervous structure of a compound eye, will vary, therefore, in terms of the distance of the object, the curvature of the superficial ocular surface, the corresponding inclination of the crystalline columns to one another, the size of their individual transverse sections, and their lengths. The continuous retinal picture in the simple eye is psychically interpreted as a continuous image. If, therefore, the possessor of a compound eye perceives a continuous image of an object, it must be the result of a more complex psychical operation, in virtue of which, the separate portions of the ocular mosaic picture are psychically combined, and interpreted as a continuous whole.

The successive researches of Treviranus, Gottsche, Hannover, Pacini, H. Müller, and Kölliker, have determined the existence and general structure of close-set rods or columns, which extend between the inner and outer surfaces of the retina, in the midst of the nervous and vascular textures of that membrane. The outer extremities of these rods present a crystalline columnar aspect, and constitute, collectively, the external layer of the retina, usually termed Jacob's membrane. The ultimate filaments of the optic nerve, after being connected in a plexiform arrangement in the ganglionic layer of the retina, terminate each independently, in the more perfect portion of the retinal field, by passing into, or becoming continuous with, the inner end or side of a rod. Kölliker considers these nodes as nervous structures, that is, as terminal portions of the nerve-filaments themselves, and holds that they constitute the parts of the nervous structure of the eye on which objective light primarily acts.

Having myself carefully examined the structures to which I have now alluded, I have been able to verify the more important anatomical details, as described by their discoverers, and agree with Kölliker in considering the rods as the primary optic apparatus. I cannot, however, coincide with this distinguished observer in holding these rods as modified nerve filaments. I hold them to be special structures appended to the extremities of the ultimate nerve filaments, and referable to the same category as the Pacinian bodies, touch-corpuscles, rods of Corti, &c.; and moreover, so far am I from coinciding with Kölliker in his speculations as to the part of the rod on which the objective light acts, that I have found myself compelled, not only from the consideration of the structures themselves, but also from the development of the eye itself, and the arrangements of the compound eye, to conceive the rays of light as acting upon the retina, not as they impinge upon it, or pass through it from before, but as they pass backward again out of the eye after reflection from the choroid.

The general aspect of the rods, and more especially of those portions termed Müllerian filaments, where they collectively amalgamate in the limitary membrane of the retina, indicate, as I believe will be generally admitted, that they consist of a modification of connective tissue, enveloping and supporting the extremities of the ultimate nerve filaments in such a manner as to form special structures, which, from their functions, may be termed *photæsthetic bodies.*

That special structures are required for the initiation of action in the filaments of the optic nerve by objective light, appears to be established by the facts, that the nervous filaments of the retina, and the cut extremities of these filaments on the stump of the optic nerve, are not affected by it, although irritation of the same filaments by electrical or other means produces subjective luminous phenomena. Subjective sounds may be produced by various modes of irritation; but actual sonant vibrations can only excite the acoustic filaments through the medium of the rods of Corti, or the corresponding terminal structures in the vestibule. Corresponding terminal structures are in like manner appended to the tactile, olfactory, and gustatory nerves, apparently for a similar purpose, to provide the necessary conditions of the initial excitement of the nervous current by those secondary properties of external bodies to which the organs of touch, taste, and smell, are related.

When the attention of anatomists was directed, a few years ago, to the structure and physiological signification of the columns of the retina by the observations of H. Müller and Kölliker, I became satisfied that those structures are not, as the latter asserted, nervous structures, properly so called, but special structures, of the same nature as the Pacinian bodies and the tactile corpuscles. I stated and explained my opinion of the nature of these bodies in a lecture on the retina delivered and reported in 1854. But I had generalized these relations of nervous filaments to special terminal exciting structures, still further, in the zoological lectures which I delivered in 1853, for my late distinguished colleague and preceptor Professor Jameson. I also expounded it at considerable length in my course of lectures last winter (1855-6). I shall now state the doctrine in general terms, not only because it is necessary for the elucidation of the distinctive characters of the simple and compound forms of eye; but also because I am anxious to put on record, by submitting it to this Society, a generalization which appears to me of primary importance in the general physiology of the nervous system. I assume, as established, the doctrine of Du Bois Raymond, that a nerve filament is capable of propagating the nervous current equally well in both directions; and that the physical and physiological characters of this current differ in no respect, are in fact identical in the so-called motor and in the so-called sensory filaments, whether special or common, I also assume as established, that the specific manner in which a centripetal nerve current is converted at the central extremity of the filament, that is to say, is physiologically reflected into motor filaments, or, psychically interpreted as sensation, depends upon the physiological or psychical endowments of the different portions of the nervous centre with which the filaments are connected. These two positions being assumed, then, I hold that, although the ultimate nervous filament may have its functional current (that is the common nervous current), excited or initiated by electrical or other physical or chemical agencies, yet this current can only be initiated or excited, for the special functional purposes for which each nervous filament is provided in the economy, by the structure or tissue with which such filament is connected peripherally. If so, then, not only are the individual filaments of the nerves of special sense provided with current-exciting structures at their peripheral extremities, by means of which alone the objects to which they are related can initiate the nerve current; but also centripetal nerve filaments of whatever kind, are provided, in their connection with the textures from which they proceed, with arrangements, by means of which alone their functional currents can be initiated.

From this point of view, every particular structure in the organism from which nervous filaments proceed to the nervous centre, may be considered with reference to the nervous system, as a peripheral nervous organ,—that is, an organ capable of exciting or initiating centripetal nerve current; which is physiologically converted, or psychically interpreted at the corresponding central organ, according to the special endowments of that central organ.

After this preliminary statement, I am in a position from which I can explain the mode in which I understand the structure and actions of the rods of the retina in the simple, and the columns in the compound eye.

1. *In the simple eye.*—A ray of light can only impress an ultimate retinal nervous filament under certain conditions. These conditions are, that it should impinge upon the distal extremity of the filament in, or parallel to, the axis of that filament, or within a certain angle to that axis.

All rays impinging on the distal extremity of an ultimate retinal nervous filament under the conditions stated I term *photogenic* rays. Rays impinging upon, or passing through, the filament in any other direction, may be termed *aphotogenic.* The distal portion of the ultimate retinal nervous filament, I distinguish as the *photæsthetic surface.*

In order that the ultimate retinal nervous filament may be subjected to the rays of light under the required conditions of vision, its distal extremity or photæsthetic surface is inclosed in a peculiar structure, consisting of a so-called *rod* or *cone* (which I distinguish as the crystalline column), and its appended Müllerian filament, with its nuclear enlargements. This structure constitutes a specific kind of peripheral nervous organ, which, from its function, I term a *photœsthetic body*.

A photæsthetic body consists of a distal segment, or dioptric portion, elongated, cylindrical, or club-shaped, homogeneous, transparent, and highly refractive, usually termed the rod or cone ; and a proximal segment or peduncle, with its nuclear enlargements, into which the ultimate nervous filament passes, and within which it apparently terminates, probably at its outer end.

The entire aspect and arrangement of these photæsthetic bodies, their predominance over the other parts of the retina at the axial spot of the eye, and the direct continuity of their stems with the nerve filaments at that spot, appear to me to indicate not only the nature of their functions, but also the general features of the mode in which it is effected. It appears to me that the rays which act upon the nervous filaments, must be such rays as the arrangement permits to pass from behind, forwards in the axis of the photæsthetic bodies. It has now been ascertained, that the quantity of light reflected, and consequently irregularly dispersed within the eyeball from the choroid, and bacillary layer, &c. is very considerable; and it consequently becomes a very important question, to determine in what manner this reflected and irregularly dispersed light is prevented from affecting the retina. The view which I have already given of the structure and probable mode of action of the photæsthetic bodies, affords the basis of a hypothesis which meets all the conditions of the question, and is in full accordance with the comparative anatomy and development of the organ of vision. I cannot interpret the functions of the structure of the retina as now determined, except by assuming that the photæsthetic columns are impressed not by the light as it enters the eye, or as it is more or less irregularly reflected and dispersed in its interior, but only by those rays which, in their passage backwards to the pupil pass along, or nearly in the axes of the crystalline rods or columns of the photæsthetic bodies, so as to reach the photæsthetic spots under the required conditions. No confusion, therefore, can result from the multitude of convergent and divergent rays which pass through the chamber of the eye, and through the retina. By this means, the numerous rays not necessary for vision, are as it were eliminated from the operation, the eye being blind to them, and affected only by such as are reflected backwards to the pupil along the axes of the crystalline columns.

2. *The Crystalline Columns of the Compound Eye.*—As stated in my lecture on the retina formerly alluded to, I conceive the crystalline columns in the eye of the insect or crab, to act in the same manner as the retinal rods in the spheroidal or simple eye. That they do so, may be held as established by the researches of J. Müller on the laws of vision in the compound eye. Müller even refers to the columnar structure of the retina, as presenting a certain similarity to the structure or arrangement of the compound eye. F. Leydig, in an elaborate memoir published in Müller's Archiv in 1855, on the structure generally of the Arthropoda, examines minutely the structure of the simple and compound eyes, and arrives at the conclusion that the crystalline columns of their compound eyes, as well as the corresponding structures in their so-called simple eyes or ocelli, are of the same nature as the so-called rods and cones, that is, the photæsthetic bodies which I have already described in the retina of the proper simple or vertebrate eye. But Leydig entirely loses sight of a fact, which if unexplained, vitiates his conclusion as to the physiological identity of the bodies in question. In the annulose or molluscous eye, whether in its so-called simple or compound form, the crystalline columns are directed like the tubes of so many telescopes towards the object, the corresponding nervous filaments passing to them from behind; whereas the crystalline rods of the vertebrate retina are directed away from the object, that is, towards the back of the eye —are in contact in fact with the choroid, while their nervous filaments are connected to them in front, that is, between them and the object.

On the other hand, if I am correct in holding that the vertebrate eye is acted upon by those rays only which are reflected from its choroidal surface, I have not only explained physiologically why its retinal columns are reversed; but I am legitimately entitled, as Leydig is not, to consider them as the homologues of the crystalline columns of the annulose and molluscous eye.

But the teleological explanation of the opposite arrangement of the corresponding structures in the vertebrate and invertebrate eye, is, in the present phase of the science, insufficient. The difference must be explained morphologically. This explanation is afforded by the different modes in which the vertebrate and invertebrate, that is, the simple and compound eyes are developed.

In the compound eye the primordial ocular papilla or convexity, which is only slightly protuberant, has its cutaneous or superficial surface immediately converted into the crystalline columnar structure, the individual columns of which are connected with the filaments of the subjacent optic nerve. The columns are all therefore directed to the object.

The primordial cerebro-cutaneous spheroidal protuberance or papilla of the simple refracting or vertebrate eye, is speedily hollowed out in front by the development in or upon it of the lens and vitreous humour, so that from a spheroidal convex surface the primordial protuberance assumes the form of a cup, with its mouth directed forwards, and its cavity occupied by the refracting media of the organ. This cup-shaped mass is the retina; the crystalline rods are not developed on its concave surface, but on its outer or convex surface, as they exist on the convexity of the compound eye, that is, in the direction of the radii of the sphere, but directed backwards, on account of the nearly spheroidal surface.

In conclusion, I may state, what appears to be the physiological superiority of the simple over the compound eye. As the simple eye is acted on by reflected light only, it cannot be disturbed by rays not required for the definition of the image. It is also arranged so as to admit of a much more delicate or minute mosaic representation of the object, from its microscopic and reversed photæsthetic bodies being in contact with the reflecting choroidal surface on which that image is formed. It moreover combines the advantages of the continuous image, formed by the lenticular structures and the mosaic image, which results from its crystalline rods.

The following Gentleman was admitted an Ordinary Fellow:—

THOMAS LOGIN, Esq., Civil Engineer, India.

The following Donations to the Library were announced:—

Monthly Return of the Births, Deaths, and Marriages, registered in the eight principal towns of Scotland, with the causes of

Death at four periods of life, for February 1857; and Supplement to Monthly Returns for year 1856. 8vo.—*From the Registrar-General.*

Journal of the Asiatic Society of Bengal, No. 7, 1856.—*From the Society.*

Proceedings of the Royal Astronomical Society, Vol. XVII., No. 4. 8vo.—*From the Society.*

Memoirs, &c., of the Geological Survey of the United Kingdom, viz.:—

1. Figures and Descriptions Illustrative of British Organic Remains, 4to. Decades 5 and 8.
2. The Iron Ores of Great Britain. Part I.—The Iron Ores of the North and North-Midland Counties of England. 8vo.
3. Mineral Statistics of the United Kingdom of Great Britain and Ireland, for 1853-55. By R. Hunt, F.R.S. 8vo.
4. Geology of the Country around Cheltenham. By Edward Hull, A.B. 8vo.
5. On the Tertiary Fluvio-Marine Formation of the Isle of Wight. By Edward Forbes, F.R.S. 8vo.
6. Prospectus of the Metropolitan School of Science applied to Mining and the Arts. 6th Session, 1856-57.
7. Collection of Maps and Sections.
8. Annual Report of the Director-General of the Geological Survey.—*From Sir Roderick Murchison.*

Almanaque Nautico para 1858, calculado de Orden de S. M. en el observatorio de Marina de la Ciudad de San Fernando. 8vo.—*From the Observatory.*

The Assurance Magazine, and Journal of the Institute of Actuaries, April 1857. 8vo.—*From the Institute.*

Memoir on the Roman Garrison at Mancunium; and its probable influence on the Population and Language of South Lancashire. By James Black, M.D., F.G.S. 8vo.—*From the Author.*

Monatsbericht der Koniglichen Preuss. Akademie der Wissenschaften zu Berlin. Juli 1855—August 1856. 8vo.—*From the Academy.*

Abhandlungen der Koniglichen Akademie der Wissenschaften zu Berlin. 1855, 4to. Erster Supplement,—Band. 1854. Folio. —*From the Academy.*

A General Catalogue of the Principal Fixed Stars, from Observations made at the Hon. East India Company's Observatory at Madras in the years 1830-43. By T. Glanville Taylor, F.R.S. 4to.—*From the Hon. E. I. Company.*

Astronomical Observations made at the Hon. East India Company's Observatory at Madras, in the years 1843-47; together with the Recomputation of the Sun and Moon, and Planetary Observations, since 1831. By T. Glanville Taylor, F.R.S. 4to.—*From the Hon. East India Company.*

Astronomical Observations made at the Hon. East India Company's Observatory at Madras. By Capt. W. K. Worster and W. S. Jacob. 1848-52. 4to.—*From the Hon. E. I. Company.*

Revenue Meteorological Statements of the North-Western Provinces, for the several Official years from 1844-45 to 1849-50. 4to. —*From the Hon. E. I. Company.*

Meteorological Register kept at the Hon. East India Company's Observatory at Madras. By J. Goldingham, F.R.S., and T. Glanville Taylor, F.R.S., for the years 1822-43. Folio.—*From the Hon. E. I. Company.*

Monday, 20*th April* 1857.

DR CHRISTISON, V.P., in the Chair.

The following Communications were read:—

1. On the recently discovered Glacial Phenomena of Arthur's Seat and Salisbury Crags. By Robert Chambers, Esq.

It will be remembered that, when the cutting was made, early in 1846, athwart the shoulder of Arthur's Seat above Samson's Ribs, for the formation of the Queen's Drive, the rock was found hollowed in a trough-form for a space of about eighty yards, and smoothed and striated in the manner of a glacier-bed of the Alps. The striæ were in the direction of the hollow, pointing to east 20° south. The whole was covered over with a brown tenacious clay, containing fragments of rocks of the district, along with some supposed to have come from a distance. The phenomena were carefully observed at the time, and reported on to this Society by our associate Mr David Milne (now Mr Milne-Home).

The formation of a road to Duddingston along the south side of Arthur's Seat has, during the first three months of this year, revealed a set of phenomena precisely similar in character, at the well-known pass of Windygowl, through which it was necessary to make a deep cutting. This pass, it may be remarked, was simply a low point or breach in the crest of an upturned bed of porphyritic greenstone which comes prominently out to the south in the form of what is called the *Girnel Crag*, and on the other side loses itself in the mass of the hill, the dip being to the north-east. When the surface matter was taken off at this place, a tenacious brown clay was disclosed, very much like that which had formed the covering of the smoothed rocks above Samson's Ribs. When this was removed, the upturned edges of the greenstone bed were laid bare; basseting of course towards the south-west; and all were found to be rounded, smoothed, and striated, the striæ lying in nearly an east and west direction. The rocky outline formed an irregular hollow, of which about six feet was thus worn, being the portion heretofore covered with clay, while about thirty feet more was composed of the rough weather-worn cliffs of the Girnel Crag on the one hand and the hill-side on the other. The only difference between the hollow here laid bare and that formerly exposed at Samson's Ribs, was that the rocks were less worn down. There was not here, as in the other case, a complete trough with smooth sides or walls, and every longitudinal chink worn, as I remember to have observed, down to the bottom, as if by some searching—I might say insinuating—agent. We only saw a rude hollow, whose irregularities had been partially ground down—the same work, as it were, half done. There was not, however, a single prominent face of rock within the hollow which did not show more or less of rounding, smoothing, and streaking. It is worthy of remark that these appearances were not confined to the immediate gorge cut in the Girnel Crag, which was not more than ten yards in extent, but were partially observed on prominent surfaces of the hill-side for fully fifty yards to the westward.

Immediately to the east of the gorge, the cutting, though descending at a rapid angle towards the lake, did not reach the rock. It presented, however, a deep section of superficial matters. First was the compact blue or boulder clay containing small stones. Next was a rough brown clay drift, containing large boulders, all rounded, and many of them smoothed and scratched. Over this lay a bed of tolerably pure sand, and over all was a thick deposit of debris from the hill-side, including many large angular masses. The boulders in the clay beds have been reported on as from the rocks of the district. One which lay in the brown drift just beyond the gorge, to the eastward, was a square mass of arenaceous limestone, probably from a bed in the hill-side little more than a hundred yards to the westward. It was three feet long by twenty inches broad, and was split up by the workmen into six slabs, exposing a multitude of the characteristic conchifers of the formation to which the bed belongs, besides a few vegetable remains, apparently calamites. What was very remarkable, this block bore the glacial dressing with striæ on two sides crossing the planes of stratification, and further seemed partially water-worn on one of its ends.

It is important to remark that the boulders were all of eastward transport, and in perfect accordance with this fact was that of the deep section of superficial matters being presented to the eastward of the gorge. A precisely similar deposit being found to the east of the Loch Crag, between Windygowl and Duddingston, we may fairly conclude that the westward side of these prominent masses was the *stoss seite* or *exposed side*, and the eastward the *lee side*, with regard to the movement of the agent by which the attrition was produced.

The operations for the new road within the garden to the north of Duddingston Church have since laid bare a sloping face of exceedingly hard greenstone, which had been only covered with a thin bed of vegetable soil. I am assured that this face, though not smoothed or marked with striæ, was dressed and channeled in much the same manner as the well-known surfaces on the west slope of the Costorphine Hill, which were first described by Sir James Hall.

It is worthy of notice that the smoothed hollow above Samson's Ribs was 385 feet above the level of the sea. The ice-worn pass at Windygowl is about 180 feet above the sea.

Glacially-marked surfaces have within the last few years been laid bare at three other places in this group of hills.

The most remarkable was at the north foot of Arthur's Seat, close beneath the line of the North British Railway, and within the precincts of the St Margaret's Station. It was a swelling piece of surface, fully thirty feet each way, and all beautifully polished and scratched, the striæ pointing to E. 15° N. Many greenstone boulders of large size, generally with flattened and polished soles, marked with striæ in the line of greatest length, were taken out of the clay-drift which overlay this surface.

About two years ago, the officers of H. M. Office of Works were good enough, at my request, to lay bare a few yards of the surface of the trap-bed near the summit of Salisbury Crags. The exposed space was found to be worn down into a smooth slope, with shallow channelings and deeply cut striæ in the line of the inclination, E. 15° N. Many of these striæ could be traced for one or two yards; and throughout a space of fifty feet along the summit of the hill, they were all of uniform character. The cliff has here been quarried away, so as to form a deep *sinus*, and, as the lines go up to the present verge, it is of course to be presumed that they had originally gone much farther.

What is, however, most remarkable in this instance, is the clear presence of a system of cross scratching, of posterior date, and quite as evidently the result of natural causes. These scratches are generally less than one foot long, and only impressed on the swelling interspaces between the channelings already described. They point to E. 20° S., being a difference of 35° from the direction of the earlier and more general striation. It is worthy of remark, that the summit of Arthur's Seat lies pretty nearly in the centre of the separating lines.

Professor Fleming having some years ago observed some glacial markings on the verge of a projecting piece of rock, at a spot called the Egg Pond, about 150 feet above St Anthony's Chapel, I had an exposure made there, by favour of the Government Officers, to the extent of two or three yards. This surface is flattened, polished, and marked with striæ pointing to E. 15° N.

The Egg Pond, it may be further remarked, beside which this smoothed surface is presented, is a now dry hollow, forming part of a little narrow valley which here indents for 150 yards the haunch of the hill. It is a circumstance not without its significance, that this little valley or trough lies in precisely the same direction as the striæ of the smoothed rock. There is a hollow precisely similar immediately under the summit of Arthur's Seat, to the north, with a ridge of equal length forming its boundary in that direction. This valley and this ridge lie in the same line, namely, pointing to E. 15° N.

On the north haunch of Arthur's Seat there are many exposed trap surfaces of a rounded form, but much weathered, and sometimes greatly shattered. These may be considered as *roches moutonnées* in a state of extreme decay.

I have only to remind the Society of what Mr Maclaren pointed out many years ago, that fragments of the trap of Salisbury Crags are scattered over the back of that hill, and in some instances have been transported across the valley, and placed higher on Arthur's Seat than any part of the parent hill now is. One has settled a little above the hollow just described as existing under the summit of Arthur's Seat. From the very hard greenstone, again, which constitutes the south haunch of the Lion, large blocks are carried eastward along the slope of the hill to the extremity of the park in that direction.

It will be observed, that amongst the whole phenomena, old and new, there are some remarkable harmonies. All the drifted matters have been carried eastward. The prominences are all abrupt towards the west, while to the eastward they are tailed away, affording on that side shelter to accumulations of loose matter. The striæ in four exposed surfaces of the two hills, and two remarkable troughs or hollows on Arthur's Seat, are coincident in direction, namely, between W. 15° S. and E. 15° N., being precisely the direction in which the striæ in numberless other places throughout this district, and all the longitudinally-shaped hills and hollows also, are disposed. Overlooking the somewhat discrepant direction of the hollow over Samson's Ribs,—for which some accidental cause may be speculated on,—this uniformity in the phenomena may be said to speak strongly for the deep ice-current, which I have long upheld as necessarily to be assumed, to account for a large class of appearances in Scotland, as I believe also in Scandinavia and in North America.

I have already pointed out, however, that ice has operated in more ways than one in our country.* The traces of a later glacial system

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* Reference is made to a paper which I had the honour to read to the Society in December 1852, and which is published in Jameson's Journal for April 1853.

—a system of local subaerial glaciers—are abundant throughout all the alpine grounds of Scotland, as well as those of Cumberland and Wales. By these the boulder clay—the result of the original and general ice-work—has been swept out of many valleys, and ordinary moraine detritus left in its stead in more partial situations. One of the most notable memorials of local glaciers in our island is the curving ridge of detritus forming the dam which retains a mountain lake. Examples are to be seen at Lochs Whorral and Brandy on the eastern skirts of the Grampians, at Loch Skene in Dumfriesshire, and Llyn Idwal in Wales. In many places, a north-looking *sinus* in a mountain has such a curving ridge of detritus girdling it in front, without any lake. Keeping in view these objects, of which I have now seen a considerable number, I am inclined to think that the valley between Arthur's Seat and Salisbury Crags has been the seat of a glacier, and that its moraine is still to be seen at the lower end, near the bleaching-green. There is certainly at that spot a broad *agger* of detrital matter, including many rough blocks, and which has all the appearance of having once confined a lake, the breach by which the water was discharged being still visible. That this lake existed so lately as the reign of Mary is tolerably well evidenced by a passage in Marjoribanks' *Annals*, where it is stated that, at the marriage of Lord Fleming to the daughter of Lord Ross, in May 1564, "the banquet was made in the park of Holyrood House, *under Arthur's Seat, at the end of the loch*, the Queen's grace being present,"—this description being scarcely applicable to any other place. It may be considered as favourable to this view of the former condition of the Hunter's Bog, that very few blocks lie there, in comparison with the multitudes which are scattered over the mountain sides. In other papers I have proved that the system of local and subaerial glaciers was preceded as well as followed by a submersion; from which it may be inferred that it was a period of elevation,—perhaps of such elevation as to bring the higher grounds of our island within the snow-line,—being all that was required to produce the phenomena to be accounted for.

2. On a Dynamical Top, for exhibiting the Phenomena of the Motion of a System of invariable form about a Fixed Point; with some suggestions as to the Earth's Motion.

By Professor Clerk Maxwell.

The top is an instrument similar to that exhibited by the author at the meeting of the British Association in 1856. It differs from it in being of smaller size and entirely of brass, except the ends of the axle; and in having six horizontal adjusting screws and three vertical ones, instead of four of each kind.

It consists of a hollow cone, with a heavy ring round the base, and an axle, terminating in a steel point, screwing through the vertex. In the ring are the nine adjusting screws, and on the axle is a heavy bob, which may be fixed at any height.

By means of these adjustments the centre of gravity of the whole is made to coincide with the steel point, and the axle of the top is made one of the principal axes of the *central ellipsoid.*

The whole theory of the spinning of such a system about its centre of gravity depends on the form of Poinsot's ellipsoid corresponding to the particular arrangement of the screws. The top is intended to exhibit those cases in which the three axes of this ellipsoid are nearly equal. In these cases the instantaneous axis is never far from the normal to the invariable plane, which we may call the invariable axis. This axis is fixed in space, but not in the body; for it describes, with respect to the body, a cone of the second order, whose axis is either the greatest or the least of the principal axes of inertia.

To observe the path of the invariable axis in the rapidly revolving body, we must have the means of recognizing the part of the body through which it passes at any time. For this purpose a disc of card is placed near the upper end of the axle. The four quadrants of this disc are painted red, yellow, green, and blue, and various other marks are added; so that by observing the colour of the spot which appears the centre of motion, and the diameter of the coloured spot, the position of the invariable axis in the body at any instant may be known, and its path traced out.

This path is a conic section, whose centre is in the principal axis. If that axis be the greatest or least, it is an ellipse with its major axis parallel to the mean axis. If the axle of the top be the mean axis, the path is an hyperbola as projected on the disc.

When the axle is the axis of greatest inertia, the direction of motion in the ellipse is the same as the direction of rotation. When it is the axis of least inertia these directions are opposite. All these results may be deduced from Poinsot's theory, and verified by means of the coloured disc.

The theory of precession may be illustrated by this top in the way pointed out by Mr Elliot, by bringing the centre of gravity to a point a little below or above the point of support.

The theory and experiments with the top suggest the question— Does the earth revolve *accurately* about a principal axis? If not, then a change of the position of the axis will take place, not in space, but with respect to the earth, so that the apparent positions of stars with respect to the pole will remain the same, but the latitude of every place will undergo a periodic variation, whose period is about 325 days. To detect this variation, the observations of Polaris with the Greenwich transit circle for four years have been examined. There appeared some doubtful indications of a variation not exceeding half a second. A more extensive investigation would be required to determine accurately the period, and the epoch of maximum latitude at a given observatory, which must depend on the longitude of the station, as the pole of the "invariable" axis travels round the mean axis from west to east.

3. On the true Signification of certain Reproductive Phenomena in the Polyzoa. By Dr Allman.

When the reproductive phenomena of Alcyonella, as manifested both in gemmation and true generation, are viewed in their proper sequence, they will be found to present a series of acts which admit of an obvious comparison with the class of phenomena commonly known as the "alternation of generations."

From the fecundated ovum an embryo is produced in the ordinary way after the segmentation of the vitellus. In this embryo, which presents at first the form of a locomotive ciliated sac, sexual organs are never directly developed, but there are produced within it by a process of *gemmation* the following series of zooids. 1. A polypide, which, like the containing sac, is essentially nonsexual, and which is eminently organized for the functions of digestion. 2. A peculiar bud, at first undistinguishable from the polypide-bud, but which never develops digestive organs, and is soon seen to be filled with proper ova, each with its germinal vesicle and germinal spot. This body, may, in accordance with common usage, be called the ovary of the zooid from which it is developed, but since it is produced from this zooid in the manner of a bud, exactly as the polypide is, it may itself be fairly viewed as a unisexual zooid, in which the whole organization becomes subservient to the reproductive function, while all the other functions and their special organs become masked and suppressed by the dominant development of the organization destined for generation. 3. Another unisexual bud developed upon the polypide, endowed with a male function and commonly called the testis, but truly a distinct zooid, with its whole organization rendered subservient, as in the ovary bud, to generation. 4. A nonsexual bud of peculiar form (the statoblast) also developed from the polypide.

The esential features in the reproductive phenomena just enumerated, present themselves in an indefinitely repeated series, where the first and last terms of each cycle consist in a fecundated ovum, and the intermediate terms in a succession of gemmæ.

4. On the Destructive Distillation of Animal Matters. Part IV. By Dr Anderson, Glasgow.

5. Analysis of Specimens of Ancient British, of Red Indian, and of Roman Pottery. By Murray Thomson.

Ancient British Pottery.

The specimen of this pottery was found last spring (1856) on the property of William Stirling, Esq. of Keir, along with the remains of a human skeleton, and so broken into fragments as to be of no archæological value

The clay, or rather loam, from which this pottery had been made had evidently undergone little or no previous preparation; the fragments were brittle, and had not been highly fired;—in this respect being inferior to the pottery of the Ojibbeway Indians about to be described. The fractured edges of the pieces presented two layers, the outside one of a dun hue, the inner black; but neither of the surfaces was glazed. Its brittleness rendered this pottery easily reduced to powder, which had a uniform olive-brown colour.

	No. 1.	No. 2.	Mean.
Silica, ...	52·49	51·24	51·86
Alumina, ...	13·29	12·46	12·87
Peroxide of iron, containing phosphates corresponding to 1·01 Phosph. Acid, and also a trace of Manganese,	18·19	18·94	18·56
Lime, ...	4·85	5·13	4·99
Magnesia, ...	0·60	1·64	1·12
Soda, ...	3·06	2·97	3·01
Potass, ...	0·55	0·78	0·66
Organic matter, . . .	2·14	2·33	2·23
Water, ...	4·70	4·76	4·73
	99·87	100·25	100·23

Ojibbeway Pottery.

The specimen of this ware which I examined, in general appearance very much resembled the Ancient British Pottery, being like it made of unprepared clay, marked on one of its surfaces by lines forming part of some simple design. In colour, the surfaces of this ware were whity-brown. The section of the fragments presented a black appearance, almost as if the clay previous to firing had been mixed with some carbonaceous substance. It was, however, better fired than the British ware, and rung to some extent when two pieces were struck together.

	No. 1.	No. 2.	Mean.
Silica, . .	42·70	43·60	43·15
Alumina, . .	22·71	22·12	22·41
Peroxide of iron, .	10·58	10·03	10·30
Lime, . .	1·33	1·46	1·39
Magnesia, .	2·60	2·88	2·74
Organic matter, .	10·28	10·10	10·01
Water, . .	9·79	9·99	9·89
	100·19	100·18	100·185

Lustrous Red Roman or Samian Ware.

This pottery has already been analysed more than once, and my analysis was only confirmatory of those already published. It would appear that in many of those pottery clays peroxide of iron can to a very great extent replace alumina, for, in the specimen I analysed, the oxide of iron is in greater quantity than the alumina; while in all the analyses of this pottery I have seen, the alumina is the greater.

The specimen of this ware which I analysed was procured from the Museum of the Society of Antiquaries, Edinburgh.

After the analysis of the mass of this pottery was finished, I scraped several of the pieces at my disposal, so as to ascertain the composition of the glaze of this beautiful ware. I could only procure enough for a qualitative analysis; but this was sufficient to show a circumstance already noticed about this pottery, namely, that its glaze contains no tin, lead, or antimony, or any of the heavy metals.

Silica, ...	54·78
Peroxide of iron, containing phosphates corresponding to 0·42 Phosph. Acid, .	21·43
Alumina, ...	8·74
Lime, ...	12·67
Magnesia, ...	1·33
Water, ...	1·26
	100·21

6. Theory of Linear Vibrations. Part VI. Alligated Vibrations. By Edward Sang.

This part of the paper contains an inquiry into the action of a vibrating body upon a linear elastic series, as representative of the action of a sound-emitting substance upon the air.

It results that when one end of a linear elastic series is attached to an oscillating substance, all the internal oscillations of which the system is capable when one end of it is fixed, are called into existence; the number of these being equal to the number of the elements in the system, and their periodic times being mutually incommensurable; and that to these is added another, isochronous and synchronous with that of the oscillating substance.

The investigation shows that the whole of these oscillations are instantaneously communicated to the system, and that the state of repose in which it was at first is merely that phase of the general motion in which all the parts but one have their velocities simultaneously zero.

On account of the incommensurability of the times, no periodic return of this or of any other phase can take place, and thus the formation of waves or pulsations in a perfectly elastic uniform linear series is impossible ; so that this line of inquiry also fails to give any indications of the velocity with which a vibratory impulse is conveyed from one end of such a series to the other end.

When the periodic time of the oscillating body is exactly equal to that of any of the internal oscillations of the system, the extent augments indefinitely with the time during which the action is continued; a result which would imply that the loudness of a sound should increase with its duration.

The attempt to pass from a discrete to a concrete system by the method of infinitesimals fails, because by augmenting the number of the parts, we also augment the number of the equations of condition, not one of which can be omitted without vitiating the result.

The general conclusions are these :—That the observed phenomena of sound are inconsistent with the supposition of a perfectly elastic vibratory medium, and that either the viscidity, or some as yet unknown quality of the air, has to do essentially with the production of those phenomena, so that any analysis in the present state of our preparatory knowledge must be futile. And that the undulatory theory of light is altogether conjectural, since far from knowing how one supposed wave would influence another, we do not yet know anything of the manner in which such waves can be formed at all.

The following Donations to the Library were announced :—

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SESSION 1857–58.

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